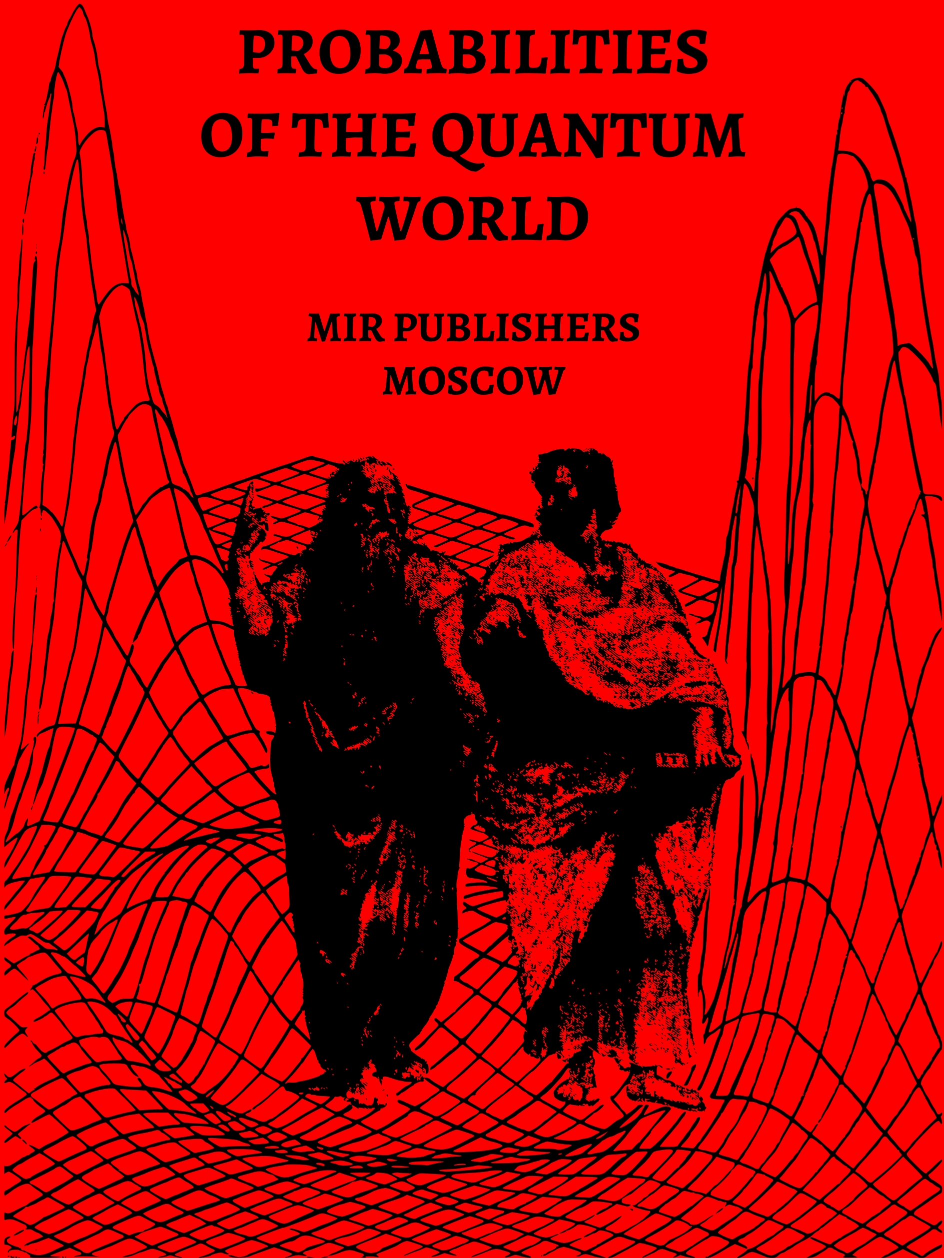
DANIEL DANIN

PROBABILITIES OF THE QUANTUM WORLD



MIR PUBLISHERS
MOSCOW

ИЗДАТЕЛЬСТВО • МИР •

Д. ДАНИН

ВЕРОЯТНОСТНЫЙ МИР

Издательство «Знание»

D. DANIN
PROBABILITIES OF THE QUANTUM WORLD

Translated from the Russian
by Oleg Glebov and Vitaly Kisin

MIR PUBLISHERS MOSCOW

First published 1983
Revised from the 1981 Russian edition

На английском языке

Contents

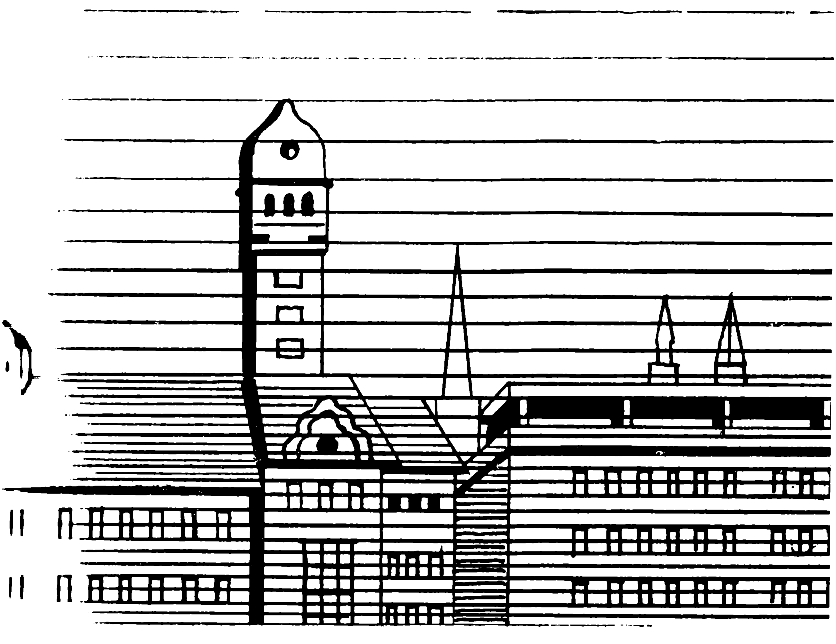
Introductory Chapter

The Archive of the Unforgettable Time

> Let us try to think well—this is the foundation of morality.
>
> Blaise Pascal (17th century)

There is an old safe rule for story-tellers—start a good story from the beginning...

Good advice must be heeded. The more so that ours is most definitely a good story—that of the quantum revolution in human understanding of the structure of nature. This was accompanied by a revolution in the style of physical thinking. It is not every century that such things happen. It all seems to be quite straightforward—this must be a documentary book



since the story itself is true. If so, then the book's structure is known beforehand; it must just follow the events of the past in which nothing, of course, could be changed. The story-teller must act merely as an understanding recorder, must he not?

It would seem he must. But there is a problem here. And not just one problem...

The first problem lies in the attitude and the extent of understanding of the recorder. Any person can miss something—sometimes almost deliberately—can undervalue some fact, record another ambiguously, become excessively fascinated with one aspect, and be indifferent to another, and so forth and so on. Even the archive files remain strictly neutral only up until the moment when the story-teller opens them. Once opened the selection rules come into effect, and he selects the events of the past for his story according to his understanding of them.

Thus, the story-teller ceases to be a neutral recorder of events—he turns into the author of a documentary. Is that not a contradiction in terms, 'author of a documentary'? Of course; but it is an inescapable and irremovable one. Any documentary writer recreates his own version of seemingly unchangeable events of the past or present. And it would be imprudent to promise complete accuracy and impartiality—that is simply impossible.

Of course, the dates, the general shape of events and their scientific meaning are inviolable for the writer. But history was (and is) created by people, each with their own unique psychology—and in psychology anything is open to different interpretation. Here nobody can safely say to the story-teller that 'in fact' something happened differently. What can take the place of a particular interpretation? Only another interpretation—nothing else. That is the nature of real-life stories.

Our good story is good just because it is precisely that—a human story. One would like to tell it not as a logically ordered account but as a mixture of ideas and passions, moments of inspiration and despair, joy and sadness...

Doubtless, it would be better to start the story from the very beginning. But here is another problem—which beginning? The story has many beginnings and any one of them could serve. Again one has to pick and choose. And again it is the will of the story-teller that is instrumental in making the choice. Can we finally become independent of it?

Maybe the solution would be to start from the end? The end must surely be as unique as the mountain summit conquered by climbers, or the victory in a battle. But another good side to our story is that it has no end, and will never have one. The outstanding ideas in science are immortal and they do not harden into a perfect mould but continue to grow in the field allocated to them by nature.

Then, how to begin? Maybe, simply...

I still remember that misty and yet sunny autumn morning in Copenhagen. A city-dweller well knows such mornings when the unseen sun shapes the indistinct magnificence of smoky pink ancient cathedrals and unrecognizable towers brought closer by the mist. Everything seems just a bit unreal. It is as if one faintly recalls the streets and squares—but what is real, and what was a dream? And will one see beyond the turning what solidly stood there in the clear air of yesterday evening? What news awaits one around that barely-seen corner? On such a morning one feels as if one has stumbled into another story.

That is how I felt that morning in Copenhagen. But I keep remembering it probably because I myself, like the city around me, was also filled with a

sunny mist—a joyous excitement. It should be explained that I had been fantastically lucky.

I was collecting material for a biography of Niels Bohr, and my fondest dream was to visit Copenhagen, the capital of quantum physics. But how could I possibly have hoped that I would be invited to work there for a month at the archive of the Bohr Institute of Theoretical Physics? That, however, is exactly what happened in autumn 1968.

That morning elderly Fru Betty Schulz, the late Bohr's permanent secretary, took me into a small room in 17 Blagdamsvej: near the window curtained with the sunny mist she handed me the latch-key to the archive door. Her thin hand caressed the wide desk and she said: 'It's a good place for work.' On hearing my awkward words of gratitude in English she added: 'Work in peace'. It was not accidental that after giving me the key she tried to help me acquire that peace of mind. Her long and extensive experience immediately told her that this short-time visitor was one of those guests who could not by themselves shake off a bothersome excitement.

During a life spent at the side of Bohr all through the history of his famous Institute, she had seen other people arrive there in a similar excited state—not the historians of science or authors coming now but the veterans of the quantum revolution, the people who had made it all possible and whose voices had since faded away.

I came to listen to the echoes of those voices in the quiet of the archive.

1

Conventional archives are collected randomly. They grow slowly in small unpredictable steps from one

find to another. Usually, there is no need to hurry. And usually, there is nobody to make haste. Archives are built up over decades or centuries.

But this archive sprang to life very swiftly: in just a thousand days almost everything destined for it, but scattered all over the world by the winds of history, was collected and brought together. This archive was programmed!

In August 1960, five university scientists met in Berkeley, California. It was our good story that brought them together. To be more exact, it was their concern that the story had not yet been written, and (to be even more specific) the scientists were concerned about the safety of its documentary evidence and determined to preserve the testimony of its participants for future generations.

It was, probably, the first time in the history of the natural sciences that such a short period—just three decades—was the object of such concern or, to put it another way, of such devoted attention.

It should not be thought that the people who met in Berkeley were retired veterans, jealously concerned about their own posthumous fame. Not at all! They were a small group of active physicist-historian-philosophers who as scientists thought of themselves as grateful heirs of the quantum revolution. Gratitude—that is the key. Their evaluation of the contribution made to human knowledge was unanimous: 'Without parallel in the last three hundred years'—these words of the prominent theorist John Wheeler, the initiator of the meeting in Berkeley, sum up the common opinion.

There is nothing exceptional about such a glowing judgement. The feelings of the people who met in Berkeley could be equally shared by all scientists

throughout the world. But these five decided to act! Their August meeting started the preparations for an extensive three-year project without parallel in the history of science archives.

The preparations were soon spurred on by a sad event: on January 5th 1961 the creator of wave mechanics, Erwin Schrödinger, died in Vienna at the age of 73. 'The bell has been tolling', said Wheeler who headed the archive committee, 'time is short.'

His meaning was clear. Everybody remembered the recent losses: Enrico Fermi in 1954, Albert Einstein in 1955, John von Neumann in 1957, Wolfgang Pauli in 1958, Abram Ioffe in 1960... And now another prominent scientist of the 'Sturm und Drang' period had departed leaving unopened a whole chapter in the history of the quantum conceptions.

Of course, much could be still learned from Schrödinger's letters, diaries, note-books, lecture notes, manuscripts, drafts—in fact, anything that might be found in his personal papers and have been preserved by his widow and his correspondents. But was he careful with his papers? did he keep the letters he received? did he keep the copies of his letters? He was said to have been somewhat neurotic. His habits were irregular. In the years of Nazi rule in Germany he led a wanderer's life—from Switzerland to Italy, and to Ireland. How many of his papers were preserved during his arduous travels? No matter what else had been preserved, Schrödinger's death meant that his live voice telling a personal history of ideas and passions could no longer be recorded.

The bell was tolling and new never-to-be-filled gaps steadily appeared in the still unwritten history, and in the still uncollected archive of documents for future historians. The latest gap left by Schrödinger's death was as conspicuous as those made by the

deaths of Einstein, Pauli, Fermi... The loss was especially grave since the main object of the archive project was to make live records of the scientists' recollections. 'That was the project's primary raison d'être', later wrote the workers on the project which was headed by the prominent historian of physics, Thomas Kuhn.

They found that almost a hundred veterans of the quantum revolution were still alive—theorists and experimenters of different calibres and different schools who had worked together in the past in the old scientific centres of Europe and USA. Now, decades later, they had to be sought out all over the globe. Take, for instance, two famous exiles from Göttingen, Max Born and James Franck: after World War II the former came to live in Bad Piermont, in south Germany, and the latter in Falmouth, Massachusetts. Thus, a hundred veterans and forty geographic locations, metropolitan and remote, from Copenhagen to Palo Alto, Rome, La Jolla, New York, Delft, Warsaw, San Diego, Kioto, and Pacific Grove; to Moscow, Vienna, Paris, London and so on.

The idea was to visit each veteran and to attempt to obtain from him as much information as possible relating to the scientific, psychological, social and everyday-life aspects of that period. Would it not be simpler to suggest that the veterans each write their memoirs? No: the project workers, themselves learned men, knew from personal experience what the results would be.

About the same time, in 1960, Soviet Academician Igor Tamm (the Nobel laureate in physics) was asked why he did not write his memoirs although he had heard, seen, experienced and done so much in his many years spent in the service of physics. 'What!' was his caustic reply: 'Can you see it in my

face that it's time for me to start writing memoirs?' He was then sixty five but the contemporary ideas in the theory of experimental particles held more fascination for his active mind than the events of the past.

In addition, it is not exactly easy for a nonliterary man to embark on the exasperating hunt for the right words in which to express himself. The seventy-year old Niels Bohr would have never written his recollections about Rutherford without the help of his young assistant Jorgen Kalkar. But how many questions one would have liked to ask the author of those remarkable pages! For instance, what did he mean when he said that he had caught onto the quantum principle in the structure of the planetary atom back in the spring of 1912 in Manchester? Here is one of the sources of the quantum revolution. But now there is nobody to ask though in 1962 it was still not too late.

Finally, written recollections are, typically, too selective, for a variety of reasons, starting from the requirements of modesty and tact, and ending with the limitations of format and size. Werner Heisenberg published a book in the sixties. But in it he never told how in 1927 as a twenty-six-year-old assistant professor, who had developed his own microscopic mechanics, he had bitterly wept at the blackboard during a hopeless discussion of the guiding principle of quantum physics. And yet such details are indispensable in recreating the atmosphere of the relentless search for the basic truths in which the veterans lived, and one has to ask them for such details, to beg them for more...

Thus, the archive committee made the only right decision: the researchers must furnish themselves with justifiable curiosity, an investigator's tenacity

and high-quality tape-recorders and talk to each veteran. But prior to that each veteran must receive a detailed questionnaire reflecting their role in the events and their assumed knowledgeability. But that meant preparing about a hundred treatises on the history of the quantum revolution. No, it is not an exaggeration—these questionnaires are veritable treatises (take my word for it, I have read many of them).

It is no wonder that the first planned interview was conducted only on February 15th 1962. But it is amazing that in less than two and a half years, on May 18th 1964, the last, one hundred and seventy fifth, interview was completed.

Why is the number of interviews greater than that of veterans? But was it conceivable to conduct a single interview when the leading participants of the quantum 'revolt' were being cross-examined?! Some interviews took a few days. Werner Heisenberg was the most generous contributor: his twelve interviews took 20 hours, and the transcripts comprise 300 single-spaced type-written pages. Niels Bohr gave five interviews: seven hours of taped conversation. Paul Dirac gave five interviews, Max Born gave three, Robert Oppenheimer gave three... This is why the number of the invaluable files in the archive's safe reached 175.

You pull out the deep steel box and see all the files with transcripts neatly arranged in alphabetical order—from the Italian Edoardo Amaldi, one of Fermi's coworkers in Rome, to the Japanese Hideki Yukawa who predicted the nuclear particles, known as mesons. The historical evidence is quietly waiting for something; one would like to say—for immortality. But it is better to say modestly that they wait for those increasingly frequent occasions when another historian or author, still under the spell of that sun-

ny mist or having escaped from it, cautiously and yet eagerly reaches for them.

Frankly, it is hard to maintain a coolly appraising attitude when left alone with these files, which together make up a kind of dramatic chronicle told by different persons. A fitting name for it would be that of Proust's famous work *A la recherche du temps perdu,* which might be translated as 'The hunt for the lost time'.

2

The two and a half years spent by the collectors of the archive in finding and interviewing the veterans could also be regarded as a dramatic chronicle, though with a somewhat different name—'The race for the time not yet lost'.

Thomas Kuhn who presided over 133 out of 175 interviews felt the tragic overtones of this race particularly keenly in November 1962. The projects workers then had just started their 'European' year and Niels Bohr invited Kuhn to establish his headquarters in Copenhagen.

The first of the six interviews of Oscar Klein, the oldest Bohr's assistant, had been recorded. They had visited Max Born at Bad Piermont. An appointment had been made to meet the head of the French theorists Louis de Broglie in Paris. An appointment had been made to interview Schrödinger's widow in Vienna... On October 7th the project workers congratulated Bohr on his 77th birthday and in three weeks they started the series of interviews with the patriarch of quantum physics, the most significant in the project.

The tape recorder reels started spinning and Bohr started spinning the tale of his long road into the unseen

depths of matter. According to their set rules, the historians started by asking him about the beginnings of everything in his life—the years of childhood and university studies.

Each of the future rebels in science had his own peculiar path of development. The historians were glad to have a chance to hear Bohr himself talking about his early life, something that nobody else could do. Among other things, he remembered how in his student's years he had been going to write 'something on philosophy'.

In fact, he was looking for a mathematical solution to the problem of free will. If everything in nature is predetermined and man is not free to choose what to do, any ethical standards are meaningless; since man is not free in his behaviour any discussion of conscience and morality is groundless. But if there is free will, how can one reconcile it to the classical determinism according to which everything in nature is governed by absolute necessity? The seventy-seven-year-old scientist smilingly said that he had been fanciful in hoping to resolve this ancient philosophical puzzle with mathematics. But what a student! Such lofty aspirations hinted at the great future ahead of him. Indeed, just by this fact from his biography we could guess his future role in subversion of the prevailing classical determinism, that is, the concept of the predetermined course of events in the Universe.

The historians, fascinated with Bohr's tale, asked for more details: he illustrated his words with pictures drawn on a blackboard; the tape-recorder reels were spinning...

The historians pressed on chasing the not-yet-lost time turn by turn as if along a twisting mountain road. The old scientist was growing tired of the incessant recalling of past events. At the end of the

fourth interview he asked for a pause. But since the way up to the summit—to the main events of the quantum revolution—remained so long he hastily added that the pause should be a short one.

The project's programme limited the interviews with the veterans to events up to the early thirties (maybe, that was the only questionable point in the entire programme) because by then the 'Sturm und Drang' period had ended. But Bohr reached only the early twenties in his recollections...

On November 17th 1962, as promised, he again embarked on his search for half-forgotten events, closely followed by Thomas Kuhn and his Danish coworkers. Now it seems symbolic that Bohr ended the fifth interview with his recollections of the Copenhagen philosopher Harald Heffding who had been his university teacher. A statue of the Greek goddess of youth, Hebe, stood in Heffding's house. Bohr recalled a peculiar remark of the old philosopher—he said that he often looked at Hebe to see 'if she was satisfied with me or not'. Some grave thought of Bohr's was felt here. Maybe his hidden meaning was whether he, in his turn, who had revealed unknown features of nature and had contributed to our knowledge of it, had earned the gratitude of the young generations?

Next day, October 18th, he suddenly had a headache, went to his room to lie down and peacefully died.

Thus, with stunning suddenness, another gap appeared in the archive. Now only documents can throw some light on what Bohr had not the time to tell. Fortunately, he left a very substantial number of papers. His scientific correspondence consists of more than six thousand letters. His early papers starting from the lecture notes from his university days to the Nobel lecture of 1922, consist of about six thousand pages.

The papers from the remaining forty years of Bohr's life had not yet been classified at the time.

3

The workers on the project knew that they would never find the documents reflecting Bohr's anti-Nazi activities in the thirties among his personal papers. He had had to burn them in spring of 1940 when Hitler's Germany occupied Denmark. Though formally these documents were outside the time limits of the quantum revolution specified by the archive's programme (1898-1932) they could have told much of the tragic fates of individuals in the years 1933-1940 against the background of the clash of scientific ideas. Hence another irretrievable loss.

In their interviews with the European veterans the historians repeatedly uncovered the deeply-etched traces left by the entanglement of the turmoil in science with the storms of history: the echoes of the latter were still to be heard in the souls of those that had suffered, even if these were the souls of 'otherworldly' abstract theorists.

Before interviewing eighty-year-old Max Born, Thomas Kuhn made the understandable decision to invite Pascual Jordan, former assistant to the head of the Göttingen theorists, to take part in these interviews. After all, he was twenty years younger than his teacher and would have a better recollection of the circumstances of their joint efforts to develop the mathematical formalism of quantum mechanics.

Pascual Jordan, by ancestry the descendent of a Spanish soldier who settled in Germany after the Napoleonic wars, did not emigrate from Nazi Germany. Now he was a professor in Hamburg and could easily

be invited to Bad Piermont. But the old teacher pointedly refused to see his former student. The angry refusal had political causes: Born could not forgive Jordan's pro-Nazi behaviour in the thirties and his support for the revival of German nationalism in the fifties. Born warned Kuhn that his wife felt even stronger about Jordan, and 'that blockhead Pascual' would never be allowed to cross their doorstep (these are the words of the archive's curator in Copenhagen). The historians had to carry out their interviews with Max Born in the presence of Friedrich Hund, another no less prominent Born student but who, being of an advanced age himself, was less suited for this supporting role. The historians came across much that unexpectedly added a human touch to the collected academic material.

The old prejudice about the ivory tower of abstract science was shown to be false. The doors of the tower were smashed wide open by the storms of history. The inhabitants of the tower turned out to be quite ordinary people, who passionately wished the world to become more humane and contributed everything they could to that end (Jordan and a few others were an unnatural and, fortunately, rare exception). Thus, in their quest for the not-yet-lost time the historians learned important things about the social and moral conditions under which the quantum revolution occurred.

4

The tape-recordings were transcribed.

The letters and manuscripts were microfilmed.

The collection of copies of the historical documents was steadily growing.

The original documents remained the property of

their owners who continued to keep them if they wished to do so.

Then all the collected documents were copied in triplicate to be kept at three equivalent storage sites of the archive. One is at the library of the University of California (Berkeley), the second is at the Philosophical Society of America (Philadelphia), and the third is at the Niels Bohr Institute (Copenhagen). Thus, another unique feature of the archive of sources for the history of quantum physics is that it is stored in three copies independently at three sites, simplifying the use of its invaluable documents.

An amazing book in a severe grey dust-jacket was published in 1967. Its 176 large-size pages do not contain any continuous text, apart from the prefaces: they are densely packed with the names of people, with references, bibliography, geographical names, cryptic abbreviations and so on. Among the multitude of names given in the book one can see those of the outstanding Soviet scientists who did microphysical research —Ioffe, Kapitsa, Landau, Skobeltsyn, Tamm, Fok, Frenkel... Among a multitude of references can be seen the titles of finished and unfinished books, delivered and undelivered lectures, published and unpublished reports... In short, it is a true reference book, a book to consult, not to read. But somehow I felt differently...

It is a fascinating guide-book for mentally travelling through the first 33 years of the 20th century, when the foundations of classical natural science were shattered and the physical concepts of the world were revolutionized. That is why physicists, historians and philosophers agree that these three decades were without parallel in the last three hundred years, since the time of Newton.

In fact, this book would serve as a good guide for our 'good story to be started from the beginning'. Of course, travel with a guide-book is more convenient, and at the same time, more troublesome. The guide-book is always silently reproachful—you have not seen this, you spent too little time here, you went astray there missing some wonderful sights... The guide-book offers dozens of tempting routes. But when a traveller has only limited means he sighs and selects one, usually the most impressive, route. Such a route does not require a guide at all. Thus, we shall not look frequently into the files containing the transcripts of the veterans' confessions, or into their letters. In fact, we shall do it fairly rarely—this is not a treatise on the history of science but a free, though nonfictional, narrative of the quantum revolution. Moreover, it touches only on its decisive events and its key heroes.

Why did I start the book from this, somewhat superfluous, tale about the origin of the archive of the sources for our good story? Because this tale immediately makes one see the vast scale of the story, and feel its dramatic and yet strikingly human character.

A year before his death Niels Bohr wrote: 'It was a wonderful adventure to live in that time...'

But when did that time begin?

The archive's catalogue, our guide-book for the period, gives the date—1898.

So be it.

In addition, a few words of self-explanation.

In my previous three books—*Inevitability of strange world, Rutherford* and *Niels Bohr*—I have already told much that is discussed in this book though it is smaller and different in conception. I hope my readers will kindly excuse some unavoidable repetitions here.

Chapter One

Two Beginnings

1

As the 19th century came to a close it looked as if it had grown dissatisfied with its well-earned title 'the age of steam and electricity' and wanted to stake a claim to the name of the atomic age. And that it did in 1895 with the discovery of X-rays; in 1896, the discovery of radioactivity; and in 1897, the discovery of the electron.

These three discoveries were indeed the sign that a new era was beginning in the natural sciences.

The first two phenomena impressed contemporaries with their dramatic mysteriousness—unknown rays penetrated opaque bodies and made it possible to see what had hitherto been invisible. It was as if nature had revealed some magical properties.

The third discovery was a low-key event and did not make much impression on the newspapers. It was one of those laboratory victories which only knowledgeable people—and not immediately and not all of them—could appreciate.

The Cambridge Professor J. J. Thomson recalled forty years after his discovery of the electron that he had made the first report about the existence of these particles during the evening session of the Royal Institute on Friday April 30th, 1897. Much later a prominent physicist told Thomson that he had thought at the moment that Thomson's report was a hoax. Thomson was not surprised, since he himself was very reluctant to give such an interpretation of his experiments; only when he was satisfied that there was no other way of accounting for the experimental data did he report that he believed in the existence of bodies smaller than atoms.

So that was what the scientist was disturbed by—the reality of bodies smaller than atoms. Then one could be disturbed by just such an admission. Could it really be that by the end of the great age in natural sciences physicists knew so little about atoms that they were not sure even about the complex structure of them? They understood almost as much about the primary structure of matter as the ancient philosophers Democritus and Lucretius who lived more than two thousand years ago (and neither of whom worked from morning till night in a laboratory).

This provides a psychological explanation for the reluctance felt by the forty-year-old head of the Ca-

vendish laboratory at Cambridge about his great discovery. In historical perspective it adds significance to his scientific achievement.

Had not the particles discovered by Thomson been predicted long before? Indeed, a hundred and fifty years back, in 1750, Franklin, famous for his lightning-rods, made a reasonable speculation that electric 'matter' consisted of extremely fine particles. In 1891, six years before Thomson's report, his compatriot Stoney gave a name to this hypothetical particle, calling it an 'electron'. Thomson did not even need a new name for his particles. Thus, he should not have been so 'reluctant' about his discovery...

His belief in the existence of electrons which he made public in 1897 still needed direct experimental confirmation. They had to be demonstrated not on paper, nor calculated from some indirect considerations, but in the real life—look at them, and count them, if you will, one by one. He found a way to do that: he managed to coat them in drops of fog which envelopes any electrically charged dust particles. In his ingenious experiments electrons vividly demonstrated their existence. That was in 1898.

And it was to be the first of many exciting discoveries of the elementary particles of matter which are still being made. Of course, the term 'elementary particle' was not used at the time.

2

Every generation of scientists experiences the ever-repeating struggle of ideas.

I wonder why the eighty-year-old J. J. Thomson did not tell in his memoirs (published in 1936) who had been the outstanding physicist who felt that the

'news' about the electron was leg-pulling? Maybe Thomson decided to save his name from our condescending smiles.

Most probably, it was the kindness of old age which looks for kindness in return. Old Thomson who outlived his entire generation was in need of such kindness. As the years passed he turned into the 'Cambridge Fossil'—as Rutherford called him—since he regarded the new ideas in physics, primarily the quantum ideas, as too fanciful. 'He placed himself outside physics', Bohr was later to say to the historians. Not hoping for the indulgence of the young, the aged J. J. continued to live as if he were outside life; and he who had enjoyed society started to live as a recluse. In his solitude he recalled the years of his glory. He felt that if he had expressed irony about the old conservatism of his outstanding colleague this would now tell against himself. That is why he did not mention the name of the sceptic.

Indeed, not to recognize the electron at the beginning of the atomic age meant to put oneself outside physics, to exclude oneself from its future. Physics could no longer continue to study nature without the concept of the electron. That soon became clear to everybody.

Soon? To everybody? No: it just seems so now.

It sounds fantastic but Wilhelm Roentgen refuted the discovery of 'the bodies smaller than atoms'! In his laboratory in Würzburg where he had recently made his famous discovery Roentgen did not allow his students and workers even to mention 'electrons'. In 1900 when he transferred to Munich this prohibition went with him. The future prominent Soviet physicist Abram Ioffe who had studied at Munich University at the time later told about the inflexible opinions of the wilful professor.

But scientific views cannot be explained by the character of the scientist in question. A capricious character can explain only how scientific disagreement turned into a prohibition. In fact, Roentgen was indeed very consistent in his actions and had a very strong will. Later, these sides of his character were even more dramatically manifested than in the case of the electron.

By the end of World War One (1914-1918) the German people were starving as Germany neared collapse. The 73-year-old Roentgen was losing his strength through malnutrition. His Dutch friends sent him butter and sugar. However, he felt that it was improper to care for his personal well-being while those around him suffered and he gave away these food parcels. And so he was slowly wasting away.

His moral sense was always unwavering.

In the last year of his life it played a sinister part in the fate of his scientific heritage. He attached importance only to completely finished research, and he firmly applied the same test to his own work. Therefore, his will provided for the burning of his unfinished manuscripts. The fire of his moral rectitude destroyed also the unfinished work of the young Ioffe which he had started together with his teacher in Munich.

We can only guess that Roentgen did not recognize the electron because Thomson's studies were, in his opinion, unfinished, that is, not convincing enough. (In other words, they should be burned and not approved, forbidden and discontinued.) It is a plausible explanation. The more so as Ioffe testified that the electron remained for Roentgen 'an unproven hypothesis used frequently without sufficient grounds or sufficient need'. Thus, the electron was not welcomed in Würzburg and Munich only owing to the

superexacting character of the experimenter Roentgen.

Maybe, that was so. But that was not the main explanation which could be gleaned from Ioffe's words 'without need'. Roentgen did not need the electron. His philosophy did not need it!

His classical philosophy of nature and philosophy of cognition could exist without this troublesome part of the material world. The beautiful and precise equations giving a classical description for all phenomena —mechanical, thermal, electromagnetic and optical— did not need bodies 'smaller than atoms'. Moreover, they did not need any concepts of complex intra-atomic structure. The laws of classical physics did not need compound atoms.

Strange as it may seem it can be easily understood. And, of course, it should not be condemned. This is an age-old wisdom.

'If we want to study astronomy', said Plato's Thymeus, we need not be interested in the celestial bodies.

Indeed, at the time only the displacements of the celestial bodies could be studied, but not their structure or composition. Only the poetic imaginaion could reach these immeasurably distant bodies. For ages the poets did what they liked with them populating them with gods, or the souls of the dead, crediting them with good or evil will. This did not have any significance for describing their motion in the sky.

In the same way, the hydrodynamic calculations for a dam are not affected by the presence of little fishes in the river.

Thus, as noted by Igor Tamm, Einstein saw that the electron was 'a stranger in the country of classical electrodynamics'. Indeed, in contrast to the classicist Roentgen, the free-thinker Einstein did not want

to deprive this stranger of a residence permit for physics: he was interested what the electron could tell about the laws of the still unknown country he came from.

Of course, Roentgen was far from thinking that the classical description of nature had been completed. His rays he called X-rays, never Roentgen rays—and it was not just his distaste of self-glorification. The letter X is used to denote something unknown. However, he was sure that with time this unknown would find a classical explanation, based on well-established physical laws. Roentgen could not conceive that X-rays were produced within atoms by such 'unnecessary' and 'unproven' electrons!

Every day the young research student from Petersburg, Ioffe, did battle for the electron in his talks with Roentgen, who excused the cheek of his talented and industrious student. Finally he—and the new physics—overcame the old man's obstinacy. This insignificant but symbolic event took place ten years after the discovery of Thomson's particles, in the year 1907, which apart from that produced nothing at all remarkable in the history of the study of the atomic world.

In that same year thirty-six-year-old Ernest Rutherford had just come to Manchester University to become the head of laboratory there; a twenty-two-year-old student at Copenhagen University, Niels Bohr, was still in his fourth year of studies; and at the University of Vienna twenty-year-old Erwin Schrödinger was in his second year.

In Paris fifteen-year-old Louis de Broglie still had another year before finishing at the lycée while in Munich a six-year-old Werner Heisenberg was playing checkers. Lev Landau was not yet born and quantum physics was in its cradle.

3

The discovery of the electron made it possible, at last, to start visualizing the atom—to develop plausible models of it.

Of course, inquisitive minds had tried to guess the atomic structure long before the 'bodies smaller than atoms' were found by experiment. But they had had no foundation to build on. Therefore, the fruits of their impatient imagination were subject neither to scientific criticism nor to scientific protection. There were no tests for checking the plausibility of any model. However, forceful intuition sometimes produced veritably fantastic results.

Here is an entry from the diary of a student at the University of Strasbourg:

> 'January 22, 1887.
>
> Each atom... is a complete solar system, that is, it consists of different atom-planets which revolve at different speeds around the central planet, or move periodically in any other way.'

The Strasbourg student, of course, could say nothing about his 'atom-planets' or the central planet. Nevertheless, he gave a brief graphical outline of the planetary model of the atom which Rutherford experimentally proved twenty-four years later.

That was a young student from Moscow, Pyotr Lebedev. Later he became famous as the first experimenter to measure the extremely small pressure of light. His notes in the diary of 1887 were to remain unknown for more than seventy years until they were published in 1960. Of course, Lebedev himself did not know that fifty years before a professor from Moscow, Pavlov, had thought along much on the same lines, or that Johnstone Stoney, the 'godfather' of the

electron, also visualized the atom in the same solar-system form. A similar astronomical image came to the mind of Nikolai Morozov, the Russian revolutionary who spent more than twenty years in tsarist jails and made highly original contributions to different sciences. The same idea came to sober-minded Jean Perrin and to many others, too, both before and after the discovery of the electron.

Before and after the discovery... But all the same each one felt as if he was the first to have had this vision in the history of knowledge. It was not the result of the continuity of thought: there was no repetition of ideas, just a few lucky people gifted with a constructive intuition who at various times had the same prophetic vision. That sounds unscientific but, in fact, it can be easily explained. Each time it was the indestructible belief of people in the unity of nature that gave birth to such a vision. It prompted a guess that the very small and the very large in the universe—the solar system and the atom—were probably built according to the same principle. That was not purely physics but basically natural philosophy which evolves much slower than science.

Those scientists who had a vision of the solar microsystem after Thomson's discovery had, of course, a great advantage—candidates had appeared to fill the vacancies of the 'atom-planets'. Why could not electrons play this, or a similar, role? For instance, at the beginning of the century a Japanese theorist Nagaoka developed an atomic model in the form of Saturn with electron rings. It did not look more fantastic than the solar model.

Naturally, J. J. Thomson who brought electrons onto the physical scene also began constructing a model of atom. He started at once, as early as 1898. But he was not tempted with profound astronomical par-

allels: his role for electrons in the atom was quite prosaic, that of 'raisins in a dough'. (It is said that it was actually he—and not subsequent writers—who made this comparison, which is somehow in keeping with the image he had at the time of being a 'jolly good fellow'.)

But if the negatively charged electrons in Thomson's atom were the raisins what was the dough? It was the atomic space itself, 'a sphere with uniform positive electrisation', said Thomson. It provided for the electric neutrality of the atom as a whole: any model had to satisfy this physical requirement.

But any atomic model had to satisfy another requirement, namely, it had to be stable, for the real atoms of the durable matter we encounter on Earth evidently satisfy this requirement. Thomson's 'cake' did not. The electron-raisins were supposedly at rest in the positive dough. But it had already been shown that any system of stationary charges was doomed to break down—the forces of the electric interaction, of repulsion or attraction, immediately remove the charges from the equilibrium state.

Thomson had to modify his explanation. After six years, in 1904, he allowed electrons to revolve in individual small groups or rings. But still the model was far from being plausible. Its irreparable drawback was the idea of positively-charged space. But the answer was still unclear, nor revealed in experiment.

Until Rutherford came...

4

He was a student of J.J.—the first overseas research student in ancient Trinity College in Cam-

bridge. When the twenty-four-year-old son of a New Zealand farmer appeared there in 1895 the old Cambridge hands looked askance at the newcomer. But soon Cambridge would hear the words of an eminent physicist:

> 'We have got a rabbit here from the Antipodes and he is burrowing mighty deep.'

True, the name 'rabbit' did not suit the New Zealander too well: he was tall, athletically built, with a booming voice. But he was really burrowing very deep—so deep that he was the first to reach the atomic depths. Not at once, of course—it was layer-by-layer work—but he had a rare insight and always managed to strike rich deposits. There are few who have had such productive scientific lives.

He witnessed the discovery of the electron. And, as another Thomson student, Strutt (the younger Rayleigh) testified, he even played a significant part in this discovery. But then the New Zealander's imagination was captured by other physical news, recently brought from France—radioactivity!

This broke new ground. And he distinguished two types of charged rays in the strange radiation of uranium, denoting them by the Greek letters 'alpha' and 'beta'. He showed that the alpha rays were beams of heavy particles with a doubled positive charge while the beta rays were beams of light particles with a single negative charge. It was he who discovered that radioactivity was the spontaneous decay of complex atoms which proceeded according to the statistical laws of chance. As a young man of just over thirty, with a still younger Frederick Soddy, he put forward and then proved the amazing idea that in each event of such radioactive decay the age-old dream of al-

chemists is fulfilled, that is, the transmutation of chemical elements.

By the end of the first decade of this century Rutherford was, probably, the scientist best prepared for an understanding of the atomic structure. Nobody had a better trained imagination for that...

The eminent astrophysicist Arthur Eddington once said at a dinner of the Royal Society that electrons were, probably, just a 'speculative concept' and did not exist in reality. Rutherford sprang to his feet and according to an eye-witness account, he looked as if ready to cry 'You have insulted the woman I love!' Actually, his words were:

> 'Not exist? not exist? Why, I can see the 'little beggars' as plainly as I can see that spoon in front of me.'

(Some ten years ago I told this anecdote to a group of scientists. Everybody laughed, apart from one young doctor of chemistry: 'Nonsense!' he said earnestly, 'our eyes are incapable of distinguishing a ball 10^{-13} cm in diameter!' And he gravely adjusted his spectacles. His neighbour laughed: 'Old chap, you never said a truer word but you are definitely no Rutherford!'

The New Zealander called the alpha particles 'little beggars'. Indeed, he seemed to have profoundly personal feelings—in fact, quite human—towards all the unseen inhabitants of the submicroscopic world. When in 1932 his student James Chadwick discovered the neutron that (the now knighted) Rutherford had predicted, and Bohr gladly recognized the reality of this newborn neutral particle, Sir Ernest in his answer cordially thanked the Dane as if there were a newborn baby in the Rutherford family. And he was always particularly partial to the alpha particles for

they had first brought him the decisively significant information on the structure of atoms. It was the alpha particles that as a fine deeply-penetrating probe helped him to 'burrow deep' in his youth...

It took ten long years to establish the main properties and the nature of alpha particles.

Their mass is four times that of the hydrogen atom. Their charge is positive and twice that of the electron: +2. Their velocity when they escape a radioactive atom is ten to twenty thousand kilometres per second. They have the same chemical properties as the helium which was first discovered in the sun spectra, and only after that on the Earth...

Ten years of work! Modern laboratory instruments would make it possible to find all these data in a month—if not in a day. But at that time the methods and instruments of atomic physics were just being born. The new ideas gave rise to new methods. The most fundamental was the study of the scattering of particles when they pass through matter.

Rutherford started these studies after eight years of work with alpha particles, in summer 1906 in Canada, where he had become the head of the physics laboratory at McGill University. His thoughts were stimulated by an unforeseen and almost unnoticeable event: a narrow beam of alpha particles that widened on having passed through a thin mica leaf. That is all that happened. But why?

The photographic plate demonstrated that a fraction of the particles had deviated by two degrees from the perpendicular. Some of the particles had possibly been deflected even more but the plate was clearly insensitive to them. Two degrees is nothing much to talk about. But it was massive microbullets that were deflected and their velocities were enormous! What could have deflected them from their straight path?

Apparently, it could only be the electric interaction with the atoms they met as they passed through a thin mica leaf. Simple calculations yielded an impressive result: it was the deflective effect of a field with the strength of 100 000 volts per centimetre. As Rutherford wrote at the time, these results clearly indicated that highly intense electric fields were concentrated in atoms.

The first (accidental!) experiment on the scattering of alpha particles showed the scientist the road leading deep inside the atom. Rutherford's unerring intuition drew him down this road.

The 'little beggars' deserved his love.

5

Less than three years later in 1909, in Manchester.

> 'He turned to me and said: "See if you can get some effect of alpha particles directly reflected from a metal surface." I do not think he expected any such result, but it was one of those 'hunches' that perhaps some effect might be observed and that in any case the neighbouring territory of this Tom Tiddler's ground might be explored by reconnaissance...'

Thus Rutherford's student, Ernest Marsden, recollected fifty years later. (Tom Tiddler's land means a 'gold mine'.) Of course, fifty years later Marsden knew very well what an immensely rich gold mine it was. But in 1909 even Rutherford himself did not quite believe his intuition. The direct reflection of alpha particles from a thin leaf of metal foil meant that they returned back to the radioactive source! Later

he repeatedly said that he had not believed it possible:

> 'It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you had fired a 15-inch shell at a piece of tissue paper and it came back and hit you.'

Rutherford did not exaggerate the incredibility of the expected effect. The alpha ray looked like a sharp arrow shot from a bow at an enormous velocity. And it was this arrow that was supposed to turn after hitting a paper target straight back to the still quivering bow-string! Why did he suggest conducting such a senseless experiment to a quite young student? Marsden was then barely twenty and at the time he was acting as an assistant to an experienced scientist from Germany, Hans Geiger (of Geiger counter fame).

One guesses here the ethical caution of Rutherford. It would be tactless to suggest seemingly senseless work to an experienced scientist—a possible failure could harm his reputation. But it is quite another matter to give the job to somebody new, for whom a failure would soon be forgotten. Indeed, the very choice of Marsden indicated that Rutherford recognized the absurdity of the suggested experiment.

Where were his prudence, his logic, his caution? It is a mystery. But maybe it is something in the psychological make-up of the great scientists, that allows them to be ever ready to forget about the recognized standards of caution and to risk everything. (Of course, such speculations do not solve the mystery but at least hint how to do so.)

However, the efforts of young Marsden were not wasted. Soon Geiger eagerly started to help his as-

sistant—in the distance they had both glimpsed 'Tom Tiddler's land'. What they saw were literally glints. To observe the scattering of alpha particles they placed a screen of zinc sulphide at various angles to the metal targets: the deflected particles hitting such a screen produced glints on it, that is, short flashes or scintillations. In a darkened laboratory the alpha particles could be counted by these flashes.

Twenty seven years later, one year before his death, Rutherford with a still fresh amazement recalled a day in spring 1909:

> 'Two or three days later Geiger came to me in great excitement and said: "We have been able to get some of the alpha particles coming backwards!" '

Thus, on that (unfortunately) unrecorded date the development of the first scientifically substantiated model of the atom was begun. And that was the beginning of nuclear physics itself.

But it was only about two years after, at the end of 1910 or the beginning of 1911, that Rutherford and Geiger reversed their roles. Now it was Geiger twenty seven years later who recalled another memorable day:

> 'One day Rutherford, obviously in the best of spirits, came into my room and told me that he now knew what the atom looked like.'

What was the work that took up almost two years separating these two symmetric scenes?

6

It consisted not so much in repeating the experiments as in the thinking and rethinking of a possible explanation for the observed miracle.

Of course, Geiger and Marsden had to count about a million flashes in the darkness to obtain statistically reliable results in these 'senseless and absurd' experiments. Only enthusiasts could do such work. As is related by Otto Frisch, Einstein was astonished by the productivity of the alpha-particle bombardment and compared it to 'shooting sparrows in total darkness'.

The well-known Russian physical chemist Nikolai Shilov who visited Manchester at that time wrote about the alpha particles in the following poetic words: they 'make a screen of zinc sulphide glitter like a feather of the fire-bird with a brilliant blue shine of indescribable beauty'. He had observed the alpha particles emitted by a source directly hitting the screen without passing through metal targets.

But a particle that scattered back, in other words in an angle of almost 180°, was a rarity—only one per 8 000 alpha particles. The blue flashes they caused on the screen always appeared unexpectedly and at unpredictable intervals. The observer had always to be ready. Rutherford himself was too impatient to do such work; moreover, his always blood-shot eyes too soon became tired: 'I damned all and retreated in two minutes...'

But he never retreated from seeking an answer to the question: what happens when a 15-inch alpha shell is reflected by the target? This incessant brainwork took up all the time. Incidentally, Norman Faser, a student of Rutherford in a later period (the

thirties), was not right when he said in his biography of the great man that there was one empty and 'wasted' year in Manchester around 1910. This is the same as to say that the time a grain germinates in soil is 'wasted'.

Faser noted that Rutherford then found himself in a blind alley. But there are two ways out of a blind alley—either go back, or somehow break through. Rutherford was one of those few who prefer to break through: it later looked as if a high wall had been demolished stone by stone.

Most of the alpha particles easily pass through the targets. It would not be so easy if the atoms were solid balls pressed from the positively charged dough as it was the case in the Thomson model. The concept of an open atom was clearly closer to the reality. By the way, Rutherford first heard about this concept, which at that time appeared under different conditions, back in 1904 in a letter from Australia written by his future friend William Bragg.

In the open atom the negative electrons and still unknown carriers of the positive charge should be at a great distance from each other. To make a comparison, it is quite easy to pass through the solar system without 'touching' anything along the way... But why nothing? And how about the force fields of gravity which act on all the bodies? Of course, one can't miss them as they fill the emptiness. Moreover, they determine the motion of the celestial bodies with their enormous masses. But in the submicroscopic world the masses are so small that the gravitational fields are not felt. The electric forces are paramount there. Rutherford had estimated these forces when they had found a deflection of the alpha ray by 2°; now he was thinking about the atomic structure which could provide for deflecting an alpha particle by 180°

or, in other words, throw it back. 'Terrifying forces must act inside the atom...' he repeated sometimes, inadvertently revealing the direction of his thoughts.

But could the reflection of an alpha particle be a result of the repeated, gradual deflection by small angles inside the target? A particle passes through a multitude of atoms penetrating the target. Each atom deflects it a bit, say by 2°. One atom deflects it by two degrees, the other by two degrees, then the third, the tenth, the fortieth... In total, 90 atoms could give the required result—180° and the particle is thrown back from the target. Could that be an explanation?

However, the probability theory was against such an explanation. It assumed a long series of deflections of the particle always to the same side, as if by special order. But there were no grounds for assuming that. Of course, a gambler tossing coins could dream of winning ninety times in a row. Why not? But the probability of such a fabulous series of wins is unimaginably small—1/2 raised to the ninetieth power. Such a small quantity cannot be compared with anything. No, the statistical laws did not allow for any noticeable deflection of a particle; the equally probable deflections to different sides would just cancel each other out. Rutherford saw that the mechanism of multiple scattering could not explain anything. So what did happen inside the target?

There was no choice: if the scattering was not multiple, then it was a single event. The reflection back was represented as the result of the collision of an alpha particle with an atom. And this—almost empty!—atom threw it back.

The light electron could not throw back a heavy particle. It was left to assume that inside the atom there was a massive nucleus which could withstand

the alpha shell. It had a small volume since the atom was almost empty. But the bulk of the atom's mass was concentrated in it. It had also the entire positive charge balancing the negative charge of the electrons roaming around in the atom.

Thus, a powerful Sun appeared in the empty planetary system. 'The enormous forces' could be attributed only to it. Those were the forces of electric repulsion in the case of the positively charged alpha particle.

The concept of the atomic nucleus was born.

This concept proved to be wonderfully useful. The smallness of the atomic nucleus explained the rarity of the direct reflection of an alpha particle: to turn back, the particle had to pass very close to the nucleus. Only then was the repulsion powerful enough. But it was very difficult to hit the tiny nucleus. This is why only one particle in approximately 8 000 managed to do that. This concept helped also to explain other features of the Geiger-Marsden experiments.

Now Rutherford had a sufficient number of building blocks for developing a plausible atom model. At the centre it had a nucleus similar to the Sun. At the periphery it had electrons similar to the planets. Electrons were attracted by the nucleus as oppositely-charged particles always are. But the electrons did not fall into the nucleus because they continuously revolve around it, as the planets do. Moreover, the velocity is sufficient, according to the laws of the classical mechanics, for the centrifugal force to balance out the centripetal force. In short, it was a precise submicroscopic model of the gigantic solar system.

Thus, one fine day at the end of 1910 or the be-

ginning of 1911 Rutherford had the right to declare loudly to his staff:

'Now I know what the atom looks like.'

7

But why did it take two years? Was not a long winter night sufficient to work out all the details discussed above (and many more we omitted)? Quite possibly; Rutherford indeed hit upon the solution in one night. It is quite conceivable that he immediately visualized the planetary atom and even saw clearly all its features. But it was just such clarity of understanding that spelled trouble—he realized immediately that within the framework of the classical laws such an atom could not exist.

He had to start thinking anew. But he kept coming back to the same dismal result. The stumbling-block was what made the planetary atom system exactly similar to the solar system, namely, the planet-like revolution of electrons around the nucleus.

Even if the rotation is steady the speed of rotation varies continuously—though its value is constant its direction varies continuously. Therefore, one of the laws of the classical theory of electricity comes into effect: when charges travel at a varying velocity they radiate electromagnetic energy. There is a precise mathematical description for the accompanying processes. But it is very difficult (if possible at all) to visualize them.

A brief aside is indicated here; in fact, it was needed even earlier . . .

Our imagination and what we can visualize with it are limited to the sphere of material entities and phenomena. The keyword here is material. However, there is a whole sphere of phenomena of a different

kind: those related to the force fields in space.

Masses give rise to the gravity fields.

Charges give rise to the electromagnetic fields.

The emptiness has a structure—it is by no means empty, and its description can be started with the term 'field'.

This term is a poetic metaphor. It hints at something monotonous and boundless surrounding all the bodies and as if planted with the forces of interaction between them—hence the term 'field'.

I recall now how we used to argue in my university years (when trying to visualize what lay behind the mathematical symbols).

Decades later, the words of a fellow student spring to mind:

'The Russian poet Tyutchev wrote: "As the ocean envelopes the globe, earthy life is entirely enveloped in dreams ..." Force fields are the dreams of matter. These are the waking dreams and they are not immaterial: "The dome of heaven in his starry glory mysteriously looks at us from the depths. And we flow on through a flaming abyss ..." '

He continued his flight of fancy saying that the 'flaming abyss' was precisely correct. It could have been a purely physical concept, rather than a poetical image since the fields stored the energy. It always can be theoretically assigned a definite temperature. And so on and so forth ...

Meanwhile another student claimed that Faraday and Maxwell should have replaced the term 'field' by the term 'sea' in their classical 19th century. It would be more descriptive and expressive. And the more so as the electromagnetic field had a wavelike nature, the other fields probably had it too. 'The sea of electromagnetic forces' has an agreeable sound to it. This sea has its calms and its storms. Then the

large physical bodies could be compared to the whales in the ocean, the smaller bodies to various fishes while the submicroscopic particles were like plankton. And to render the picture completely natural the waves and bodies must move. Finally, a field is flat while the sea is three-dimensional. And so on, and so forth...

But a third voice—that of one of our charming girl-students—disagreed with the first and the second claiming that such comparisons were too material and too rudely distinguished between matter and the fields; but matter itself is permeated with force fields, it is they that hold together everything of which the bodies are made—the stars, the atoms, the atomic nuclei... When we divide matter into smaller and smaller units the smallest particles are indistinguishable from the fields themselves; probably the elementary particles are just lumps of the field matter, 'the hunchback field' in the words of Einstein. Thus, it was not the fields that were the dream of matter; rather, matter was the dream of the fields. And a heavy dream at that (both literally and figuratively). She continued much longer in the same vein. There is no stopping the really keen student!

It was true that the more material a comparison the less it helped the imagination. But that was not unexpected. The concept of force fields did not spring from the every-day experience of our life. It appeared when physics went deeper into the unseen essence of physical phenomena.

It is often said that in our century—during which the relativity theory and quantum theory were born—physics has ceased to be immediately clear.

Yet was it really clear before? Was it easier for the human imagination to visualize the physical world in Newton's time? Then the interaction be-

tween distant bodies was explained as 'action at a distance'—through the emptiness, without mediating agents. How could one visualize that? The protracted disputes about the concept of emptiness started as far back as ancient times and continued during the Middle Ages. This concept was replaced long ago with the parallel concept of the 'world ether'. But the ether, which was assumed to be all-permeating and permeable to everything, continuous and immaterial, absolutely stationary and experimentally non-cognizable, was even less imaginable than the unsophisticated 'emptiness'.

The force fields were declared to be the different states of this hypothetical world medium—the ether vortices, stresses, vibrations. That was the explanation given by Descartes to gravity in the 17th century, and by Maxwell to electromagnetic interaction in the 19th century. But the clearness of such explanations was all but illusory since the ether itself was quite unimaginable.

Humanity had long outgrown the ancient belief in the crystal celestial spheres to which the immovable stars were attached (one had to explain how they held on!). But when the mythical celestial spheres had faded away the stars were not only left in the sky but at last obtained all the rights of an independent physical reality. Something similar happened to the ether and the force fields.

At the beginning of this century the ether disappeared from the physical picture of nature. The relativity theory demonstrated that nothing in nature was absolutely stationary. This term appears now only in the rare (and always unsuccessful) attempts to re-introduce it. But force fields, as the stars, remained. Moreover, it was only now that they acquired a genuine physical reality. The energy of all physical

interactions lies in them. They have the status of an independent form of matter. Our 'sense of nature' (if not our imagination) finds it easier to deal with the current concepts—immaterial ether, like the emptiness, was a heavy and unnecessary load.

Our understanding has not become less clear, since its former clarity was only imaginary...

It should be added here that Einstein said the following words which echo those of Rutherford in defending the reality of the electron: 'For the contemporary physicist the electromagnetic field is as real as the chair on which he sits'!

How can we imagine how the electron-planet revolving around the nucleus-sun according to the classical laws must lose the energy of its motion in the emission of the electromagnetic waves? Radiation, of course, is not free; energy should be expended for it. The force field generated by the electron or generating it is not immaterial, it has a mass; and thus it has a physical inertia. While the electron travels along a straight line at a constant velocity its force field obediently follows it or travels with it. But when the interaction with the nucleus makes the electron turn from the straight path the field is swept off, as by the train of a gown. Depending on its inertia the field resists the variation of velocity and 'breaks off', or is radiated in the form of electromagnetic waves...

Such a visualization is really not worse than any other that could be invented. Any one would be naively rough and open to criticism

Actually, the only significant consideration here is that the energy of electron motion in the planetary model could counterbalance the attraction of the nucleus: but this energy must be continuously spent for radiation to occur according to classical laws. If it is

so, the electron will inevitably fall into the nucleus. Each 'electron-planet' would have this fate.

Rutherford understood that what he saw was a doomed atom.

8

But what then of his confident words to Geiger, that he knew what the atom looked like? One should appreciate the subtlety of these words: he did not declare that he knew the structure of the atom, but just what the atom looked like—nothing more. But to say even that one first had to make up one's mind to break with classical laws.

It took almost two years to choose between the path of boldness and that of humility. Humility was equivalent to a recognition of failure: a report about the results of so many years of work would then include only the experimental data and ignore the planetary model. But boldness was to insist on that model in defiance of logic but with the approval of intuition. As in any psychological conflict the outcome was determined by the bent of the personality.

Rutherford behaved according to character: in May 1911 he declared to all physicists from the pages of the *Philosophical Magazine* published in London that he knew **what the atom looked like.** But he warned at the beginning of the report:

> 'The question of the stability of the atom proposed need not be considered at this stage ...'

That meant: 'Ladies and Gentlemen, I appreciate that my atom would not stand your criticism at the moment. It is doomed. But our science still has tomorrow as well as today!'

He ended this warning with the words:

> '...the stability will obviously depend upon the minute structure of the atom and on the motion of the constituent charged particles.'

That was a research programme for the future.

Atomic physics could take this as its guidelines, or it could ignore it. To do the latter it would be enough to disbelieve the system 'nucleus + electrons'. There is direct evidence that at the beginning the most prominent theorists and experimenters of the time ignored this programme. In the autumn of this same year 23 outstanding physicists from Europe came together for the first Solvay Congress in Brussels.

(The twenty-fourth participant of the congress was its sponsor and founder Ernest Solvay, an old industrial chemist, a worker's son who was fond of science and fascinated with the mysteries of the structure of matter. It was his generous donations that gave rise to the still living tradition of the Solvay Congresses, which incidentally played a significant part in our good story.) Of course, Rutherford was also invited to that congress of 1911. A month and a half later he wrote to his friend William Bragg:

> 'I was rather struck in Brussels by the fact that continental people... do not worry their heads about the real cause of the thing.'

But the physicists who did not 'worry their heads' at the first Solvay Congress were Albert Einstein, Max Planck, Hendrik Lorentz, Henri Poincare, Marie Curie, Paul Langevin, and Walther Nernst... Such a disparaging remark about this highbrow congregation indicates that the rabbit from the Antipodes' was quite hurt.

But what had happened in Brussels?

Nothing happened! That was what infuriated Rutherford. His impatient hopes were not realized there (hopes are always impatient if they have long been patiently nurtured). It was half a year since the May issue of the *Philosophical Magazine* had appeared but nobody said a word about the planetary model of the atom in Brussels. It looked as if it was one of those failures which are so clearcut and unforgivable that they pass tactfully unmentioned in the scientific community—particularly, if a highly esteemed scientist has failed.

Of course, the atomic structure was discussed at the congress. Lorentz himself, the author of the theory of electrons (he was the chairman of the congress) talked about the advantages of the atomic model suggested by Sir J. J. Thomson. On the other hand, Arnold Sommerfeld, a theorist from Munich, disappointedly declared that he did not want to deal at all with any 'partial hypotheses about atoms'. Nobody at the congress even noted the discovery of the atomic nucleus and the development of the planetary model of the atom. Thus, the congress in Brussels did not regard these events as the starting points towards an understanding of the subatomic world.

But, maybe, Brussels was an exception? No, at that time apparently, a planetary atom did not attract anyone anywhere. Strange as it may seem, even the former student from Strasbourg Pyotr Lebedev did not voice his approval which would surely have been expected. In 'The advances of physics in 1911', a short article by this outstanding Moscow scientist, neither the atomic nucleus nor Rutherford's model of the atom were even mentioned. Of course, the character of his article could account for that—he wrote it for the general public and therefore

discussed only the indisputable and comprehensible advances made during that year. And who could call the planetary model indisputable and comprehensible at the time? We have just seen how the matter stood.

But one should say that almost nothing discussed at that congress in Brussels was beyond dispute or readily comprehensible. The theme considered was 'Radiation and quanta'.

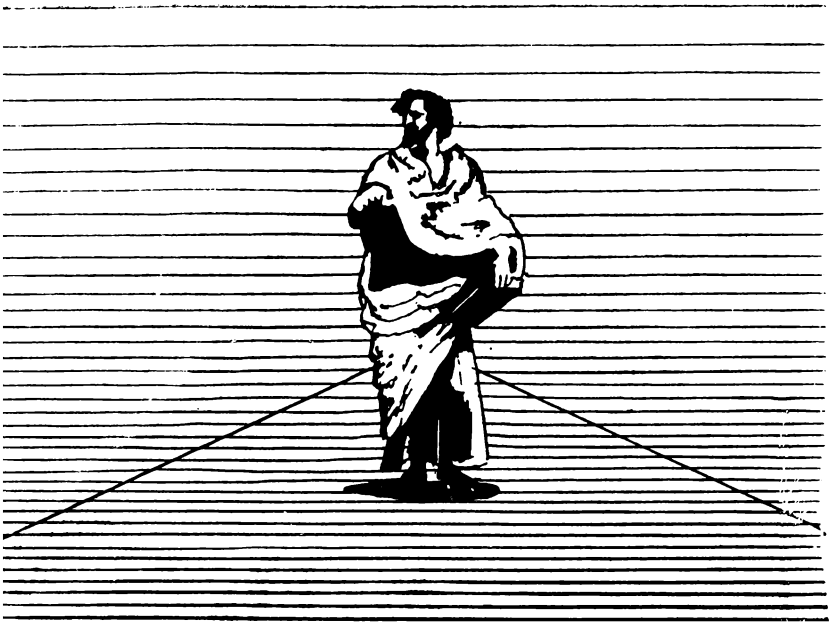
Chapter Two

Two More Beginnings

1

The word 'quantum' was born together with the 20th century—it was first heard in the quiet of a scientific meeting on December 14, 1900 in Berlin—the traditional meeting of the German Physical Society. The new word was introduced by Professor Max Planck.

Apparently, he did not at all have the feeling that he had opened a new era in the natural sciences. Planck was born in 1858 and at the time of his great-



est achievement he (like J. J. Thomson) was in his forties. And just like Thomson, he immediately felt that this scientific discovery was a burden. And the more fundamentally new the discovery was, the heavier the burden. It should be recalled that three years before—in April 1897—J. J. Thomson had 'reluctantly', rather than exultantly, told the physicists in London of the unquestionable reality of the electron; now—in December 1900—Planck told the physicists in Berlin about the enforced introduction of the 'quantum' concept with the feeling that he had been driven to 'an act of hopeless despair'.

His attitude at the time is the more understandable if we recall that he was a perfectly respectable and quietly meticulous scientist. His personality directly refuted the popular and romantic misconception that only the born rebel could question the most long-standing values of science.

'Act of despair' was not an affected phrase for one who was always reserved. And his proposal was not a sign of recklessness: he named it, extremely modestly, 'a mathematical technique'. In a letter to the skilful experimenter Robert Wood, Planck wrote that it was a purely formal hypothesis and, frankly, he did not expect anything exceptional from it; the only thing that he wanted was a positive result at any price.

At the time he had already spent six years looking for a unified formula to account for the energy distribution in the spectrum of the electromagnetic radiation of a heated body (in the ideal case). Separate formulas had earlier been found for the extreme cases of the long and short waves but a general solution had not yet been achieved by anyone. He also was unable to reach this goal until ... Until he saw that he could successfully resolve this question

if he assumed a very strange thing: that the light was emitted and absorbed in separate portions!

That was his idea. He called this portion of radiation a 'quantum' (from a Latin word meaning 'how much').

Of course, everything was much more complicated—and more dramatic. Planck could not immediately appreciate his idea in full. He was already using the idea for his theoretical calculations but he did not give it a free rein. One can draw comparison to a true image of reality having been exposed on film but remaining unseen until it has been developed. Two decades later (in his Nobel lecture) Planck himself said that the first step leading to the understanding of the nature of the quantum of energy was made by another scientist, not himself. For the sake of justice, it should be noted that the scientist who took the decisive step gave all the credit to Planck: it was Planck, he wrote, who had introduced the new hypothetical element into physics—the light quantum.

In any case, we must give the reader a simple explanation of this new concept in physics.

Field and matter are continuously interacting with each other, and hence exchange energy. There is no interaction without such an exchange. This process seems always to be continuous in terms of our experience.

When a body emits light it gives to the surrounding field a part of its energy. But it can transfer a bit less or a bit more of its energy, or even a bit more or still a bit less—and still a bit more or less. Generally, this 'bit' can be as small as desired. When a body absorbs light it can also absorb from the field any portions of energy. Thus, we can cut a piece of any length from a thread or we can drink any amount

of water from a glass. In short, things that are really continuous can be divided into any number of parts. And energy was always thought to be a continuous physical entity that could be divided up as desired. Physical processes always appeared to be continuous, and hence were divisible into steps as small as were desired.

This axiomatic belief which never seemed in need of proof (and thus was never proven) had the force of a principle of science: nature does not proceed by leaps and bounds (or, in Latin, 'natura nihil facit per saltum').

But Planck had to admit the inadmissible for classical science—that energy was emitted and absorbed in entire quanta! A body could not emit one quantum of light and a little bit more; it was either one quantum or two, three, a hundred, or a million quanta—but not a million and an eighth, or a quarter. Fractions of the quantum did not exist. The exchange of energy between the field and the body lost its continuity.

This assumption had to be made in order to derive a correct formula for the energy distribution. The experimental data then confirmed the correctness of the assumption. Many years later when telling of his historic report on December 14th, 1900 Max Planck recalled with a quiet satisfaction that the next day in the morning a fellow scientist had sought him out and told him that after the meeting, late at night, he had compared Planck's formula with his own experimental data and found a heartening agreement everywhere.

There is nothing a theorist likes to hear more.

But this deserved triumph for the classicist Planck was secretely poisoned by the anti-classical nature of his successful concept. He did not at all want to

question the principle of continuity in physical processes...

2

Planck had retained his faith in this principle from his youth: two decades earlier in 1879 in his doctor's dissertation he wrote that the atomic concept of the structure of the matter resulted in contradictions, He had assumed that the divisibility of the matter could not be limited. And now he himself had to admit that the divisibility of energy was limited!

It looked as if the quanta were the absolutely indivisible lumps of radiation, the smallest of the possible lumps of light. They were the true 'atoms' of the electromagnetic fields, in the primary sense of the word. One could not say to console or to justify oneself, 'as if they were atoms'. No, for though back in 1900 nobody knew anything about the structure of the atoms of matter, it was still clear that they had some structure; and, thus, they were divisible. But the quanta were envisaged by the theorist as something definitely not divisible: otherwise, they were not needed in the theory.

Planck had one way out of his dilemma—to deny the physical meaning of his concept by recognizing only its auxiliary (mathematical) value. In other words, when introducing the quanta into the formula where they were necessary he did not propose to introduce them into nature! It was a 'working hypothesis', nothing more, just the 'scaffolding' (that was all). And that was what he did.

(For the sake of diversion, we can imagine how the Benedectine Berthold Schwartz who invented the gun powder said to the people round him after he made

an explosion: 'No, it was just a joke!')

Even ten years after his great 'act of despair' an ageing Max Planck cautioned the young Ioffe to be very careful with the concept of the quanta: 'Don't go any further than is absolutely necessary' and 'Don't question light itself'. The latter words meant: do not imagine that light is indeed a flow of quanta.

Planck did not change his views until the end of his life; he died after World War Two in his late eighties already fully aware of the physical reality of radiation quanta, monstrously demonstrated to a shaken mankind by the atomic explosions at Hiroshima and Nagasaki. Fifteen years after Planck died, Niels Bohr commented to the historians in an interview given shortly before his own death:

> 'In some sense it can be said that he used the last forty years of his life, not to say fifty, to try to get his discovery out of the world.'

Feeling that this statement was somewhat strong, Bohr added that Planck at least was in the end satisfied with his discovery. But how about his trying 'to get his discovery out of the world'? 'Well, but it came to that', Bohr mildly said.

Fortunately for our understanding of nature, the life of remarkable ideas does not entirely depend on the intentions or lack of will of their originators. Planck in the first decade of this century warned the young Ioffe not to question the nature of light just because a few years before such questioning had been attempted by another young scientist—and successfully, at that.

That young scientist was Einstein. It was his 'first step' which Planck later recalled in his Nobel lecture.

The twenty-six-year-old expert of the third grade in the Swiss Bureau of Patents somehow desperately needed what Planck was afraid of, namely, 'to go further'. In 1905 he published in the seventeenth volume of the German *Annalen der Physik* three papers which found an immortal place in the history of natural sciences. The first opened the way to the final proof of the atomic structure of matter. The second consistently presented the foundations of the relativity theory. And the third paper was an introduction to the physics of the quantum theory of light.

Einstein dared to proclaim the physical reality of quanta. He said that they were particles of radiation, literally, small bodies 'localized in space'. This definition implies that in their motion in space the quanta occupy a certain localized region. That was what made the Einstein's idea so bold.

Thus, the concept of quanta has undergone the following transformations.

In 1900 Planck announced to the physical community that the bodies emitting light would deliver it to the theorists only in certain portions!

But that limitation was only for theorists: in fact the quanta did not exist, and one could not possess them—they are dispensed by an unknown natural mechanism at the moment of emission, but immediately blend into a continuous light flux. A quantum is nothing more than a drop falling into the ocean where it immediately loses its separateness, its localization, and its drop-like nature.

In 1905 Einstein suggested a different nature for the emitted quanta: they were minute bodies that preserved their integrity in space!

Light is emitted in portions not for one instant only—and not only for theorists—but indeed exists as

a flux of quanta. This is shown by the laws of the photoelectric effect, that is, the generation of an electric current by light incident on a metal.

We cannot explain the experimentally observed laws of this phenomenon if we imagine that the light washes away the electrons floating in the metal much as the sea waves gradually wash away the shores. But these laws can be easily explained if we assume that the light knocks out the electrons, rather than 'washing them away'. Light hits matter like a rain storm. The lucky drops-quanta collide with electrons they meet along their path and transfer their energy to them. The energy of the quanta and of the collision probabilities is just sufficient to provide for the observed flow of electrons—the photoelectric current.

Having become Einstein's particles of light, Planck's 'portions of radiation' revealed some features of the corpuscles of matter!

But, unlike Rutherford, Einstein did not say that he knew 'what the quantum looks like'. He did not search for objects to compare the quantum with—a pellet, an arrow, or a wave crest. He did not visualize anything of a mechanical model type. He was satisfied with the conclusion that the quanta of the classical electromagnetic field manifested the properties of conventional particles.

How simple, and how difficult to understand! Einstein set a baffling task for our imagination. One wonders how his own imagination could live in peace with the prospects opened by his discovery...

That was what had happened in the physics of the submicroscopic world before the mystery of the stability of the planetary atom emerged; in five years (and two instalments) the theory of quanta appeared to become, in the words of Max Planck, a source of continuing torment for the scientists.

But why torment?

But first—to prepare an answer—we shall talk about something else...

3

Did Einstein feel at least a twinge of alarm when he made his revolutionary discovery? or did he, in contrast to Planck, remain calm?

It seems that he enjoyed his theoretical visions in solitude, sitting in the boring office of the Bureau of Patents or roaming the cheerful streets of the Swiss capital. He was never in the least annoyed by the manifest discrepancy between his visions and physical good sense. One feels that only his victoriously care-free genius could have displayed the amazing fruits of his thought to the methodical readership of *Annalen der Physik* in just one year.

One hesitates even to describe his unexpected and profound inventions as the fruits of prolonged thought. Among the physicists of this century he is the one best described by the words someone once used of Leonardo da Vinci: 'Great force combined with lightness'. In 1905 he was just twenty-six and just had not had the time for the tediously lengthy process of growing fruit. It was the power and freedom of thought rather than the days and hours spent in thought that were decisive. Indeed, one is astonished more by the freedom than by the power, more by the lightness than the force.

Many statements of the relativity theory sounded like unexpected 'inventions' to the sober-minded physicists. And some of them were to play an extremely important part in our good story.

Mass had always been regarded as an unchangeable property of each body while it retained its in-

tegrity. But now it appeared that mass was relative—there was one mass at rest and another mass in motion. Mass increased with the increasing velocity of the body. But why had nobody ever noticed this in centuries before?

The explanation is that the mass increase is negligible as long as the velocity of motion is small in comparison with the velocity of light: and therefore it is quite imperceptible in our world of slow-moving and heavy things. In our daily life even the velocity of sound, 340 metres per second, seems enormous. But this velocity is lower than that of light by a factor of almost a million—light moves at 300 000 kilometres per second. According to Einstein's law a modern supersonic aircraft increases its mass during flight by approximately one trillionth of its initial mass at rest, on the runway. Say the mass of the aircraft is ten tons, then one trillionth (10^{-12}) is one hundred thousandth of a gramme. How can one detect or measure such a small quantity? Naturally, in centuries past when land vehicles travelled quite slowly no observations could prompt to scientists to conceive of any possible dependence of mass on velocity.

But what was it that gave this idea to Einstein? Not, of course, direct observation. It was pure logic, based on the non-classical features that he had revealed in the structure of time and space. But it was a very fine and precise experiment that helped to reveal these features.

In 1881 (Einstein was then a boy of two living in the southern German city of Ulm) a physicist from Chicago Albert Michelson conducted several sensational optical experiments. He determined the velocity of light propagating from a source on the Earth in two opposite directions—in the direction of the motion of the Earth, and in the reverse direction. The Earth travels along its orbit at a velocity of thirty

kilometres per second. It was expected that in the first case the velocity of light with respect to the Earth would decrease by this value (300 000 − 30), and in the second case it would increase (300 000 + 30). But the measured velocity proved to be identical in both cases! It did not matter whether the source was following the light it emitted or was moving away from it. (In fact, in the second experiment Michelson measured the velocity of light in the direction perpendicular to the velocity of the Earth, so that the source was as if at rest and the velocity of the Earth could not therefore affect the velocity of light. Of course, that did not change the result of the experiment.) In short, the results meant that the velocity of light was independent of the motion of its source!

This was in glaring contradiction to classical mechanics.

But it was easier to question the correctness of the experimental results than the omnipotence of the formulas. The measurements were repeated with ever increasing accuracy, but the results did not set minds at rest—on the contrary, it became increasingly clear that Michelson was right. For more than twenty years the cleverest theorists were advancing complicated hypotheses to help Newtonian mechanics out of trouble and meanwhile the boy from Ulm grew up without showing any particular promise. The cleverest scientists did not dare to take the simplest and the most difficult step, to recognize that Newtonian mechanics, great and perfect as it was, had reached the bounds of its applicability. Clearly, what had been revealed was a class of motions which was not governed by this mechanics. A new mechanics was needed of which the Newtonian would be just a part, one that correctly described only some of the laws of nature.

This new mechanics was brought forward by the relativity theory.

It immediately became a subject of common talk; sensible people agreed that it was terribly obscure, in fact, beyond comprehension. Some did not even stop at calling it nonsense, or even rubbish.

Even thirty years later an older Einstein complained and warned at the same time:

> 'One cannot regard a concept as senseless only because it differs from classical physics.'

At the age of sixty-eight he wrote in his autobiography about one idea, that he had no arguments to defend it apart from 'the belief in the simplicity and the intelligibility of the nature'. Throughout his life he remained true to his beliefs. What was despised as nonsense—the fruit of his early work—was also born from his philosophical belief in the harmonic simplicity of a true picture of the world. This belief cautioned the young Einstein against the intricate justifications that classical theory employed when it could not handle the difficulties of physics. Any contradictions could be resolved for a time with the help of contrived assumptions. 'But has that anything to do with nature?' was his silent question.

This is not a rhetorical question, used here for the sake of convenience. It almost exactly repeats the words written by Einstein in explaining the motivation for his search to readers of the London *Times*.

When he was a student he also tried to work out classical justifications for Michelson's results and for Planck's hypothesis. He did not tell in his autobiography how he did that, but it is remarkable that he also could not escape the temptation that he condemned at heart. The victorious lightness of his results of 1905 now seems to have its dramatic prehistory.

It is quite conventional though, if one is allowed to call conventional the 'pain of heart-felt thoughts' (Niels Bohr). These were also a few months of distress when the twenty-one-year-old Einstein tried to find a classical explanation for the inexplicable constancy in the velocity of light, and for Planck's quanta:

> 'All my attempts to apply the fundamentals of physics to these results failed completely. It was as if I could not find solid land to put my feet on and build on.'

Finally, he twice experienced Planck's feelings:

> 'Gradually I began to despair... The longer and the harder I tried, the more I felt that only the discovery of a general... concept could lead us to reliable results'.

However, his despair was of a different quality than Planck's. The latter was not ready at all to part with the classical ideal of the description of nature, while Einstein was perfectly prepared to do so. Trying to 'unearth the true laws' by old means he, in fact, was secretly searching for a new general principle of mechanics. And what is most unexpected, he had been searching for it for quite a long time: 'I arrived at such a principle after ten years of thought...'

Ten years? But if he found it when he was twenty-six that means he started his search at sixteen. Yes, exactly so. In spite of giving a superficial impression, the work of thought was indeed long. His persistent search for a new general principle was led by his implicit belief in the simplicity and the intelligibility of nature.

Indeed, only after he found a firm footing with the principle he had been seeking did he start to enjoy

his theoretical visions in the Patent Bureau office and in the streets of Bern. The principle was beautifully simple, and could be expressed in a few words—time and space are relative. But this short statement is meaningless without lengthy explanations.

4

The classical description of nature was based on the belief that time and space are absolute.

That meant that there was a single Clock for the whole Universe beating the same time for all observers—the same time for everybody!

Similarly, the absolute character of space meant that there was a single standard of length in the Universe applicable to all distances on all scales.

This unchangeability—complete independence!—of the running of the clock measuring time, and of the length of the rulers measuring space would seem beyond question and not subject to any discussion. An unsophisticated person even now would ask, and how could it be otherwise?

There is a well known and often repeated fantastic story about the 'Einstein twins': one of them goes on a space voyage at a velocity close to that of light and after two years returns to Earth, where he is met by his twin who in the meantime has aged by 20 years. Neither of them are surprised since when they parted they knew that the time in the spaceship is slower by a factor of ten because its velocity is so much greater than the velocity of the Earth. And if the spaceship had a velocity even closer to that of light the twin in it would travel an even shorter time (say a year or a month) according to his clock. But

his twin left on the Earth would still live the same twenty terrestrial years during this short interval.

This story, which could well be realized in the future, is fantastic only because humanity so far cannot build spaceships travelling at velocities close to that of light. In all other respects, it is quite true. Even that both twins, the young one and the older one, will not be surprised is also true.

The people of the future will learn from the cradle that time and space are relative, as we and our ancestors knew from the cradle that both were absolute. Just think: this familiar story of the twins is still known as the 'clock paradox', while paradox means some statement incompatible with the conventional understanding of things. Calling a scientific truth a paradox inadvertently reveals that it is still too novel for us.

The relativity theory retains a flavour of this novelty, though the 'boy from Ulm' is long dead and the laws of nature he uncovered are daily put to practical—and very efficient—use in particle accelerators, nuclear reactors and many other apparatuses. Where then is the novelty? It would be better to talk about the 'outdatedness' of our mentality which is difficult to eliminate and which feeds all our beliefs and prejudices. Among them is the unchecked feeling of the universality (the total applicability) of our terrestrial standards, seconds and metres. Our daily experience seems to vouch for it.

It was not without reason that Einstein wrote:

> 'Forgive me, Newton; you found the only way open in your time to a man of the highest scientific capability and strength of thought. The concepts created by you are still the leading ones in our physical thinking, though we now know that if we aspire to a deeper under-

standing of the interrelationships in nature, we shall need to replace these concepts with others farther from the sphere of our immediate experience.'

Having asked forgiveness from Newton, Einstein as if asked all the inhabitants of the Earth to forgive him—all of us who in our daily experience are permeated with the hereditary Newtonianism, the muscle-bound philosophy of our motions and of the visible mechanics of the things that surround us.

All the participants of the quantum revolution would be well advised to ask a similar forgiveness from their contemporaries: it has led us even further from ourselves, even deeper into a sphere of natural relationships of which we have no awareness in our daily life.

And where did Einstein derive the relativity of time and space from? He was instructed by nature.

Nature told him that the velocity of light was the same irrespective of whether the source was following the light or going away from it. In short, the velocity of light was independent of the 'reference body'. The young Einstein then declared that the constancy of the velocity of light was a law of the nature.

His decision was forcefully simple: he based his new mechanics on an unquestionable fact, but one which contradicted the mechanics of both Galileo and Newton. This immediately eliminated the need for justifying the strange fact—it was automatically included, as a postulate, in the description of the motion of material bodies. True, there is a historian who states that Einstein did not know of Michelson's results at the time. Quite possible. At any rate, he never questioned the constancy of the velocity of light—and that was what mattered.

He moved away from the experimental data towards abstract reasoning. It was a small step but it led so far that neither Antoon Lorentz nor Henri Poincaré who had prepared the way for the relativity theory could lay a clear road towards that physical frontier and thus they could not develop that theory.

No motion can be described without choosing 'reference bodies' for time and distance. In the Chicago experiments the 'reference body' was the source of light, first when it followed the light beam together with the Earth at a constant velocity, and then when it travelled in the opposite direction also at the constant velocity of the Earth. Though there was but one source in the experiments there existed as it were two 'reference bodies' for measuring the velocity of light, with the Earth carrying the source playing the part of both. Indeed, Michelson performed two experiments within the framework of one, while in principle he could have made both measurements separately. Then it would have been clear and beyond need of any discussion that he had measured the velocity of light with respect to two 'reference bodies' which executed rectilinear and uniform motion with respect to each other, by inertia. But the light acted as if it did not feel the changes in motion. Its velocity behaved as if charmed—and remained constant.

One might object that the Earth travels not by inertia but under the effect of the Sun's attraction; and its path is not straight but curvilinear, since its orbit is an ellipse. That is true. But the radius of its orbit is so great (150 000 000 kilometres) and the period of revolution so long (a year) that the motion of that Earth can be easily regarded as rectilinear and uniform during the very short time taken by the experiment.

There is a great variety of 'reference bodies' or 'reference frames'—we can describe a motion with respect to anything. However, for a long time physicists have distinguished 'reference bodies' that travel by inertia. These are called the inertial or Galilean reference frames. They have one highly pleasing aspect which is, moreover, psychologically attractive...

One can easily imagine an observer-physicist sitting on such a 'reference body' armed with the ruler and the clock observing and describing the mechanical events in the world. He does not experience any effect of the external forces himself and does not have any effect on what surrounds him, otherwise he would immediately cease moving by inertia. In other words, his presence in the world has no effect on the events occurring there or on their description. If we imagine another observer on another inertial 'reference body', we can say the same about him, as well as about a third, a fourth, and so on. All the descriptions will have the same validity—all will establish the same laws of nature expressed in the same analytical form. In other words, all the inertial reference systems are equivalent.

The classical physicists made one exception, however: they assumed the existence of one 'reference body' that was at absolute rest. Religious people called it God. Poetically minded people visualized the principal observer sitting on a celestial throne. Physicists preferred to talk about 'absolute space' or 'absolute ether at rest'.

Incidentally, the Michelson experiment was aimed at finding the answer to the question, does an 'ether wind' flow towards the Earth as it travels through the ether at rest? Thus a runner in a sports hall creates a head wind for himself though the air is at rest. Light was regarded as vibrations of the ether and

the ether wind' blowing towards the travelling source of radiation should have decelerated the light beam. The constancy of the velocity of light put an end to models of the 'absolute ether at rest' and to the concept of light as an 'ethereal phenomenon'.

But the proponents of scientific myths are fanatically unyielding. Even twenty years after the relativity theory had been born—twenty years!—a physicist Miller declared that he had detected a breath of the ether. And he seriously noted that the wall of his high-altitude laboratory had a window specially made to allow the ether wind free access to the instruments. It was this redundant feature that unwittingly compromised his favourite concept of the all-permeating ether (if it is all-permeating then why the window?). At the beginning of the thirties a German scientist refuting his results could not resist a joke at his expense: 'Unfortunately, Miller did not indicate whether the opposite wall had another window to facilitate the passage of the ether draught'.

The future President of the USSR Academy of Sciences Sergei Vavilov angrily criticized these unreasonable experiments. He was annoyed because the people engaged in those ether games were secretly hoping to remove the keystone of the relativity theory and thus bring the whole structure crashing down.

The young Einstein generalized Michelson's results and came to the first postulate of his mechanics: the velocity of light is the same in all inertial reference systems, i.e. those moving at a constant velocity.

This classically absurd postulate was added to the second postulate which sounded self-evident even to a 'classical' ear: the laws of nature have the same analytical form in all the inertial reference systems.

What logically follows from these postulates—the

dependence of mass on velocity and much else—can still make our poor imagination boggle. . .

5

If the velocity of light is the same for all observers moving with respect to each other (300 000 kilometres per second), then their standards of length and time cannot be the same. Otherwise an observer following the light would see that the light has passed a smaller distance per second than that observed by the observer moving away from the light. They would get the same results for the velocity of light only if the instruments of one of them were faulty. But both might insist that their instruments are extremely accurate: in that case both should recognize that a miracle has happened. But the discrepancy of results is too systematic for a true miracle. The 'errors' are imaginary here, for they are sanctioned by nature. But how? Both observers must agree that they have no common standards for distance and time. Their instruments measure different miles and seconds. What remains to be understood is how these differences depend on their own velocity with respect to the source of light.

. . .There is an anecdote about Einstein: a tram-conductor in Berlin once became exasperated with him and asked, 'Can't you add up?' In reply he simply offered a sheepish smile.

One can just imagine what the tram-conductor would have said if Einstein had added to his arithmetical mistake the following mysterious communication in a hushed voice: 'You know, I would like to warn you that my pocket watch—this one—becomes a bit slower in your tram than it was at the tram

stop, and the tram-car becomes a bit shorter than it was and—please, believe me!—that's the honest truth.' One hears her sympathetic voice: 'Yes, yes, dear. Those heartless people at the hospital, letting you out without an attendant! Is there somebody to look after you at home?'

But it was not a kind-hearted conductor, but angry university professors who dared to say something of the kind about Einstein, though not sympathetically. One of them—a deservedly well-known physicist, Nobel prize winner, and head of an institute at Heidelberg—Phillipp Lenard (a poker-faced man with the cruel look of a Jesuit) began saying something much more vicious when he tried (long before Hitler) to rouse 'nationalistic' feelings against the 'non-Aryan' creator of the relativity theory. That was nothing new for him, though: prior to Einstein, his victim was the perfect Aryan Roentgen whose fault was that he had successfully discovered the rays which Lenard, conducting similar experiments, had missed though he had had the chance. He nearly charged Roentgen, that paragon of honesty, with plagiarism. Later, the Nazis rewarded him; X-rays were renamed 'Lenard rays' in the Nazi literature on physics (yes, there really was such a thing!). Another Nobel prize winner, Johannes Stark, also an outright racist, followed closely in his footsteps in vilification of the relativity theory.

Einstein jokingly called the clique of irresponsible enemies opposed to his physical ideas 'The Anti-Relativistic Co. Inc.' Back in the thirties and fourties the shares of this company were rather highly priced, but after the defeat of the Nazis they plummeted in value...

By simple mathematical treatment the universally constant velocity of light can be shown to imply that

if an observer travels with respect to another observer, his clock will be running slower and his meter stick shorter. The time dilation (or 'slowing') is a general effect while the body contraction occurs only in the direction parallel to that of the relative motion.

Einstein would extract even more sympathy from the Berlin tram-conductor if he hastened to add: 'But no, don't be too disturbed—the width and the height of your tram-car are the same as at the tram-stop, while the length, sad to say, is somewhat shorter, and again—I give you my word—that's the truth!'

If the twin in the 'clock paradox' who remained on the Earth could observe his rocket-riding twin he could see that the latter was considerably flattened. This effect would disappear only after the spaceship had returned to the Earth; then both twins would have the same, old, standard of length because they would be at rest relative to each other. Their clocks would be synchronized again, too.

A natural question is why the 'flattening' of the space-going twin has not left any permanent trace when he returned from his trip, while the dilation of time in the spaceship had led to the very tangible result that the twins are now people of different ages?

The cause is the profound difference between space and time: while space has both directions—there and back, time has only one direction—there. The twin from the spaceship could return only to the same point in space, but he returned to a different and twenty-year-older Earth. The clocks became synchronized again but time itself had elapsed and could not be returned. The twins embraced each other at the same point in space where they had parted but at a different point in time.

A further question is a much harder one to answer

(and indeed the theorists had considerable trouble answering it). Why cannot we exchange the twins in the above arguments? Any motion is relative. Then why can we not say that the twin in the spaceship is at rest while the twin on the Earth is travelling at an enormous velocity? Then the results will be reversed and the terrestrial twin would remain young while the space-going twin would be older by twenty years. Otherwise, isn't the 'clock paradox' an illusion? One would recognize indeed that it was so if the twins could really be exchanged. But that could not be done—the Earth and the spaceship are not equivalent. We assume that the motion of the Earth is constant (since the period is very long we take into account only its motion with the Sun ignoring its revolution around it). But the spaceship was not moving by inertia: when it started its flight, when it turned back and when it returned to the Earth it was decelerated or accelerated, in short, what had happened to it had not happened to the Earth. Therefore the life-stories of the twins are not equivalent and what we can say about one cannot be said about the other.

Another (familiar) question is, why nobody had noticed the relativity of time and space in the past? The answer is the same as for the 'unexpected' increase of mass with velocity—the explanation lies in the mathematical law of the dilation of time and the contraction of length.

The mathematical law here works the same as for the increase of the mass: if the velocities are small in comparison with the velocity of light then the effects are extremely small. We have mentioned earlier that a supersonic airliner increases its mass during flight by a trillionth of its mass when on the runway (the 'mass at rest'). Similarly, the clock in the cabin of the airliner will be slower by a trillionth

than a clock on the ground; and the length of the airliner in the direction of the flight decreases by the same imperceptible fraction. Of course, the respective decrease in a barely crawling tram-car would be even smaller; Einstein could keep on insisting that he was telling the truth, but there were no instruments sensitive enough to obtain the experimental evidence to prove him right.

When, on the other hand, the velocity of a body approaches the velocity of light the consequences predicted by the relativity theory become substantial indeed. The mass of the body grows enormously; time slows down inexorably; and the length contracts catastrophically. The simple formulas, not unlike those in school algebra problems where the most complicated operation is the extraction of a square root, start to show the astounding consequences of the relativity theory. If a rocket could reach the velocity of light this would involve at least three impossible events:

—its mass would become infinite;

—time would stop inside it;

—its length would be zero.

Obviously, time cannot stop and the third dimension cannot disappear; the inevitable conclusion is that no physical body can have a velocity equal to the velocity of light.

It can approach this velocity but it cannot reach it. The velocity of light is the limit for the physical velocities in the Universe.

The impossibility of reaching the velocity of light is demonstrated even more clearly by the increase of mass with velocity. Even a tiny electron, whose mass at rest is almost imponderable, would have infinite mass if it could be accelerated to the velocity of light.

Energy must be spent to accelerate a body; the more massive the body, the higher the energy spent for its acceleration. The accelerators of charged particles—these mammoths of modern technology—are very large and complicated because, among other reasons, they must accelerate particles to enormous energies. The accelerator at Dubna (USSR) accelerates hydrogen nuclei (protons) to a velocity which only differs by 1% from the velocity of light; to achieve that the energy of 10 billion electron-volts must be transferred to them. Then their mass increases ten times and becomes comparable to that of carbon nuclei; but infinite energy would be needed to accelerate one solitary electron to the velocity of light!

It is only in fairy tales that anything is possible. That is why grown-ups love them: they console them in their powerlessness before the laws of nature. As for the writers of science fiction (rather than fairy tales), they could send their heroes on space trips at unlimited velocities only until the year 1905—until the 17th volume of the *Annalen der Physik* had appeared; authors who continued to do so thereafter were now writing pure fantasy, rather than science fiction.

Thus, any body that has a mass at rest cannot reach the velocity of light. But what about the Einstein 'particles of light'—the quanta of the electromagnetic field? They are no phantoms, they are quite material. Of course, they are. But the fact is that they have no 'mass at rest'. They then weighth nothing. That sounds unnatural: surely if something material has no mass it simply does not exist? Quite true. But that only means that the particles of light do not exist **at rest**. Light cannot be decelerated or accelerated! It is the embodiment of perpetual motion

with a velocity that is the highest possible with respect to any observer. And the particles of light—the quanta of energy—do have a real mass during their motion: it was real enough for Pyotr Lebedev to measure the pressure of light.

6

Like the story of the Einstein twins, this is an often repeated tale. But not infrequently one hears a sincerely puzzled or condescending voice ask, 'What! You do not believe in faster-than-light travel?' (As if it were a question of belief or disbelief!) Sometimes the question is formulated in a more artful way: 'Look, I can think in an instant of travelling to the Sun: so doesn't my thought travel faster than light?' Light indeed needs more than eight minutes to travel from the Earth to the Sun, while here it happens instantaneously. But the deluded questioner forgets that no physical process has occurred between the Earth and the Sun; the biophysical or biochemical event which occurs has taken place within our heads (and our heads are small). Therefore the shortest instant is sufficient for an imaginary journey to any other place: to the next room or to the Magellanic Clouds; to the past or to the future; to a castle in Spain or to Fairyland. The biological processes do not need the velocity of light to take place in our moderately-sized brain.

'But how about tachyons?' asked a young ESP scientist talking to a young physicist (a highly unusual occurrence since the ESP people typically know all the answers and particularly do not like discussions with physicists). 'But how about the tachyons?

I am sure that it is precisely these wonderful particles, travelling at faster-than-light velocity, that produce the telepathic phenomena. If so, then telepathy proves the reality of the tachyons, while the tachyons prove the reality of telepathy!'

Leaving aside the amusing illogicality of this tempting logic, I should remark that I have recorded it here, word for word, as I heard this argument in summer 1978 and then again in summer 1979—but that time the question was asked by a middle-aged philosopher and directed at a similarly middle-aged natural scientist. The word 'tachyon' appeared in the 70s. The *Fontana Dictionary of Modern Thought* gives the following definition:

> '*Tachyon*. A hypothetical particle travelling faster than light. Because of the 'light barrier' of relativity theory tachyons could not be produced by the acceleration of ordinary particles. Tachyons would generate a special kind of radiomagnetic radiation but this has not been detected.'

Thus the concept may be used, successfully or otherwise, by modern thinkers. The tachyon hypothesis is fascinating in that it might have appeared on any day after 1905—or it might just as well have not. For physicists did not need it. Typically, hypotheses are not born lightly—just recall Planck's 'act of despair' and the 'gradual despair' of Einstein. But this particular hypothesis stems rather from a joyful confidence in the relativity theory than from any need to 'amend' it. The tachyons are just a disinterested spiritual entertainment.

Indeed, a mathematical transformation of the Einstein formulas is possible in such a way that the ve-

locity of light becomes the lower, rather than the upper, limit of the admissible velocities of motion. Then all velocities below that of light (rather than above it) become forbidden. Thus we enter an imaginary 'topsy-turvy' world. Tachyons are the imaginary particles for such a mathematically feasible universe.

But these particles are hardly useful for telepathy. According to the thoughts of the young ESP expert and the middle-aged philosopher, tachyons should serve as the carriers of mysterious information between human beings. However, these particles definitely cannot be emitted or absorbed by bodies made up of atoms of the matter from which we know we are made. In our world, where faster-than-light velocities are impossible in physical events, all the laws governing physical interactions would have to be changed to provide, first, for the generation of the tachyons and, second, for their information-transfer role.

J. J. Thomson once said, not without sarcasm, that nature was apparently not created to accommodate the mathematicians. One could add that it was not created to accommodate the physicists either, and even less so, the ESP people—their research efforts will become really arduous only when they definitely know what they have to explain...

When Einstein was making his excuses to Newton he was sixty-eight. Forty-two years had passed since the relativity theory had been formulated. Its statements were long proved to be true; atomic physics and astrophysics had been using its equations for several decades. But Einstein, talking to Newton across the centuries, still thought it necessary to assure his great predecessor that 'the concepts you created are still the leading ones in our physical thought'!

Why such assurances? Had not the new mechanics put an end to classical mechanics?

That is the same as to ask whether motion with velocities close to that of light has replaced conventional motion. A silly question, is it not? So classical mechanics is still very much alive and with us: only now we clearly know the limits of applicability within which it can bring fruitful results. But these limits stretch so far that even space science does not need the Einstein equations for its precise calculations of the trajectories of satellites and space stations. For the time being the fastest spacecraft fly at velocities that are slower than that of light by factors of tens of thousands. Equations based on the old well-tried classical concepts are accurate to the eighth or ninth decimal place.

Moreover, apart from the fact that it is still widely applicable, the old mechanics transferred to the new one its age-old language of knowledge. And together with this language it transferred a wide range of continuing concepts. In 1905 Einstein did not have to start inventing new terms: the relativity theory could be written and was written in the terms of classical physics.

In fact, the only new and decisively important addition to the old language was the very name of the Einstein mechanics, the 'relativistic' mechanics. Physicists started to talk about relativistic equations, relativistic mass, relativistic momentum, relativistic particles, and so on and so forth. Thus, the addition of this adjective as it were doubled the size of the former language. It was a great leap forward in the correct description of the nature.

If only our imagination could make such leaps!

Relativity theory appeared at precisely the right

moment, when the scientists were starting to probe the subatomic world where almost nothing could be described using the old terms without the addition of the new adjectives—quantum and relativistic... As the Russian poet Tyutchev wrote: 'We cannot know what our words will wake...'

7

One is tempted here to recall two short scenes from Tolstoy's *War and Peace*. Of course, Tolstoy, as Tyutchev, said nothing about physics but they said much about us. Both scenes have to do with the difficulties our imagination experiences when it is divorced from logic; and the difficulties in our logical understanding when it is not assisted by the imagination.

Sixteen-year-old Natasha Rostova bubbling with the events of each day and her vague feelings says to her mother—the sensible kind-hearted countess—about her former sweetheart Boris Drubetskoi:

> 'He is very, very nice! But just not to my taste—he is as narrow as the grandfather clock... Don't you understand? You know: narrow, grey, light...'
>
> 'What a fancy!' said the countess.
>
> Natasha continued:
>
> 'Can't you see? Nikolai would understand... Bezukhov is blue—dark-blue with red—but he is square...'

What could the countess answer? How could she understand Natasha if the conventional logic of everyday life has no 'narrow, grey, light, square young men, or 'dark-blue with red' ones?

But if 'Nikolai would understand' then there should be something of the kind. Of course, there is—here is Natasha who understands that 'something'. But what about us? Whom do we agree with: the old unimaginative countess ('fancy!') or young Natasha who charmed the entire world? ('Can't you see?') Naturally, one is tempted hurriedly to tell Natasha that one understands her perfectly and—like Nikolai—is absolutely with her (one wants to be at one's best before her). Of course, what one understands or pretends to understand may be quite different from Natasha's understanding. But it is not important; Natasha's is a strange knowledge, and not obligatory for anyone: it is precise only while one has faith in the insights of her childish spirit.

But that is only a simplification.

The imagination is, of course, a free bird. But we rarely recognize that it takes flight from a cage which is ever expanded by our sober-minded understanding of reality. The imagination repeatedly returns to this cage for food. The independent and high-flying imagination is, in fact, fed—and thus, tamed—by earthly logic... In this talk with her mother Natasha just gave her logically thought-out opinion: favourable to Bezukhov and unflattering to Boris. This is why she asks: 'Can't you understand?' In fact, she has grasped the essence of both of them, and she is even convinced she is correct. She is just a little bit sly, for she does not want to pass final judgement, to make final decisions. So she presents her opinion in a form that is unfit for discussion and can only be immediately accepted as a revelation. Natasha's words baffle the countess who cannot trace a relationship between her images and logic.

In a later scene in the novel Natasha finds herself in a similar situation. In these famous scenes Tol-

stoy gives the ironically demeaning, 'alienated', description of an opera performance:

'... Everything seemed queer and amazing to Natasha, in her serious mood.

...Then more men rushed in and started to drag away that girl who first had been in the white dress and now was in a blue dress. They couldn't haul her away at once but first they sang for a long time together with her and only then dragged her off...'

Why did the other people in the audience applaud while Natasha felt that everything was preposterous and shameful? Maybe, she just could not understand what was happening in the opera? No, Tolstoy specifically mentioned that she knew the contents of the opera beforehand, and thus, she knew the meaning of the events in the opera. Maybe, the explanation is that she had imagined these events differently on the stage. She even had an urge 'to leap over the footlights and herself sing the aria the actress was singing.'

The countess had managed to restrain her 'common sense' in that night conversation. In the opera house Natasha had to restrain (though it was not for the better!) her truthful imagination.

Yes, she had to! For merely a girl, she could not for long resist the high society atmosphere of faked delight that dominated the opera house. Her watching the surroundings, rather than the stage, had its effect. After the third act 'Natasha no longer found that queer'. Finally:

> 'The curtain went up again... Natasha, now completely under the spell of the world she was in, returned to the box and her father. Everything that happened before her eyes seemed to her now quite natural'.

Incidentally, can one not see in this comparatively early scene from the novel the future of Natasha Rostova, the well-behaved banality of her grown-up life? A constrained imagination is a misfortune that cannot be put right.

But has all that anything to do with us?

Anyone has the right to reply 'No, nothing!'

But science is a human enterprise. In the crucial periods of its history the quarrels between logic and imagination are very similar to those happening in everyday life though we do not notice, or do not recognize them.

Chapter Three

An Encounter of Ideas

1

Let us return to Brussels in autumn 1911—to the first Solvay Congress where for four days twenty three outstanding European physicists discussed the radiation quanta.

It will be enough to spend just a minute at the conference. All we need to hear are just a few words by the chairman, Lorentz. Einstein once wrote of him, 'He was a quietly confident master, of himself as well as of physics'. But at that particular mo-

ment Lorentz was not exactly confident and quiet, especially when he made what sounds quite an unscientific statement: 'We cannot help feeling that we are in a blind alley.'

It is true that he called the quantum speculations of Planck and Einstein a 'ray of hope' and an 'elegant hypothesis concerning the elements of energy'. However, in the same breath he voiced his objections against this 'elegant hypothesis': it could not find any place in the physics of which he was such a 'confident master'.

It appeared that the ray of hope did not illuminate the darkness of the blind alley so much as it dazzled the scientists, bewitching them with its mysteriousness. They had to think to understand how these queer 'energy atoms'—radiation quanta—were born. The new principle of discreteness had somehow now to be reconciled with that of continuity in all physical processes.

The scientists had to comprehend the physical meaning of a new fundamental constant introduced in Planck's theory, the quantum constant h.

This constant specified the scale of the divisibility of radiation. When we multiply this constant by the number of the oscillations of the electromagnetic field per second we obtain the magnitude of the quantum: the higher the frequency of the radiation the larger, the more energetic the quantum. The quantum of green light is larger than that of red light, and the quantum of ultraviolet light is larger than that of green light while a quantum of X-rays is greater than any quanta of visible light.

Each quantum has its own oscillation frequency ν but they share a common scale of divisibility and smallness. This is the smallest physical action in nature. Planck introduced his constant h to describe

this most minute action. That is why it is called **the elementary quantum of action.**

It is an unimaginably small quantity—$6 \cdot 10^{-27}$ when measured in gramme-centimetre-second units. Obviously, such a small quantity can be measured only indirectly. But different experiments performed over the years have yielded approximately similar values for it.

Max Planck poetically described the quantum of action as 'the mysterious ambassador from the real world'.

This mysterious ambassador appeared late in 1900, presented his credentials to the scientists and started, at first timidly (and then increasingly persistently) to claim the indisputable right of the quantum country to have a proper place in the physical map of the real world. And eleven years later the great Lorentz said that 'even sceptics must recognize' this country.

He couldn't help counting himself among the sceptics, however, and despite everything continued to refer to the theory of quanta as 'a hypothesis'. And. what was most amazing Planck himself still talked in Brussels about the 'mysterious ambassador' who had come to him eleven years before as of a purely hypothetical figure.

The adjectives 'mysterious, hypothetical' could equally be applied to the velocity of light, since it emerged as a universal constant c in the relativity theory.

Up until then the velocity of light was merely one among an infinite number of physical velocities, for example the velocity of sound; nobody saw anything special about it—it was just another velocity, if an enormously high one. Suddenly it became the highest of all possible velocities and an amazing-

ly constant one at that. Before it had been measured with an ever increasing accuracy; now repeated analyses were made, with ever increasing thoroughness. But nobody among the outstanding classical scientists dared to call it a hypothesis, even in its role of world constant. Only the shareholders of the 'Anti-Relativistic Co. Inc.' were arrogant enough to do that (but who cares about the racket made by swastika-bearing physicists?).

This constant of the relativity theory perhaps had better luck than the quantum constant because the velocity of light in vacuum had been the subject of physical experiments from the end of the 17th century; at the time of Newton a scientist from Denmark, Romer, was the first to make an estimate of its order-of-magnitude.

But the quantum constant had no such past history comparable to that of the velocity of light—in fact no previous history at all.

Maybe that was why at the first Solvay Congress the brainchild of Einstein and that of Planck did not enjoy equal rights? The participants talked in no uncertain terms about the *theory* of relativity but with misgivings about *a hypothesis* of quanta. Yet historically both were of the same age and both had sufficient experimental confirmation.

Another feature one could not help noticing was that the two world constants c and h (i.e. the velocity of light and the quantum of action) had a profound similarity. This was not obscured by the fact that the first constant is an enormous quantity while the second is extremely small; on the contrary, that was what the similarity was about—both were at the extremes of possibility in nature.

The first constant was the upper limit and the second was the lower limit. One was the limit of physi-

cal velocities, the other was the limit of physical action. Neither could find any place in the traditional description of moving matter; classical science could not predict such limits. According to the classical laws the largest physical quantities reached to infinity while the smallest physical quantities tended to zero. But the new theories set two very definite and fundamental boundaries—there cannot be any velocities higher than c, and no action values can be smaller than h.

Perhaps an explanation is in order here.

Though the quantum of action did not have (and could not have) any prehistory, the concept of action in general could boast of a substantial prehistory. It had become one of the fundamental concepts of mechanics from the middle of the 18th century onward. Those who first introduced it into the description of mechanical events possessed a very good insight indeed.

Expressing this concept in the modern language of physics, we can say that nothing in nature happens without the expenditure of energy and time, but what is most important is the product of the energy which is spent and of the time that has elapsed. Obviously, a low energy performs the same action during a long time as a high energy during a short time. Thus the product of energy by time is called, very graphically, the 'action'.

In the 18th century a well-educated captain of the dragoons Pierre Louis de Maupertuis, preferring quiet days of research to a military career, formulated the principle of least action:

> 'If a spontaneous change occurs in nature the required amount of action is the least possible.'

A ray of light striking the dense material of a transparent lens is refracted in it at such an angle that the expenditure of energy and time, taken to travel in the glass, is the least possible.

A stone freely dropping in the gravitational field of the Earth travels along the most economic path—straight down. This principle is satisfied in nature always and everywhere. Maupertuis himself gave it a physical foundation—the wisdom of the Supreme Being—the creator of the Universe; of course, that being would dislike an unnecessary expenditure of time and energy (though it is not quite clear why the 'allpowerful and immortal' should care about such trifles). The former dragoon captain passed away under the watchful eyes of two Capuchin friars believing that he had revealed the administrative secret of Providence. A hundred years later, in the 1840s, the Maupertuis principle became a guiding law of classical mechanics owing to the work of two outstanding mathematicians: William Hamilton in Dublin and Mikhail Ostrogradsky in Petersburg. All the equations of motion could be derived from this principle.

Thus the words 'the least action' appeared in physics long before Planck. But they did not then have any relation to the concept of a regular divisibility of nature. On the contrary, classical action varied continuously and could easily reach zero. In short, the Maupertuis principle cannot be included in a prehistory of the elementary quantum of action.

However, there was an obvious question to ask: if the classical principle of the least action is universal, and applicable to light, to a stone and to anything in nature, why could the non-classical quantum of action also not have a universal significance? Then h would be the universal constant not only for all

types of electromagnetic radiation but for all the force fields in general, as well as for matter.

Planck was inclined to suggest a similar generalization at the first Solvay Congress. However, he still called the quantum of action 'hypothetical'.

Yet the profound similarity between the two fundamental constants called for a similarity in their theoretical fate. The discovery of the limiting velocity c led to the development of new—relativistic—mechanics. The discovery of the limiting action h could not help but lead to the development of a new—quantum—mechanics. One could even have predicted that both mechanics with time would amalgamate into one—quantum-relativistic—mechanics, having the same validity as classical mechanics but applicable on a much wider scale.

However, nobody could look that far into the future—no quantum mechanics had yet appeared. The chairman of the congress, Lorentz, had just noted the need for its development. Here he pronounced the second phrase for which we dropped into Brussels in autumn 1911 just for a minute:

> 'It is quite probable that while we are together discussing this problem a thinker in a remote corner of the world has worked out its solution.'

Remember this phrase, though at that time nobody did so...

Moreover, none of the scientists, whether participating in the congress or not, could assume (apparently) that the path of development of the new mechanics would lead through the atom. And, of course, nobody could think that it would be the 'impossible', classically-doomed, planetary atom of Rutherford. In

autumn 1911 none of the scientists began preparations to follow this road or even knew that they were already on it...

Twenty-six-year-old Dr. Niels Bohr from Copenhagen was bored with his fruitless stay at the Cavendish, the laboratory headed by J. J. Thomson; an eleven-year-old schoolboy from Vienna, Wolfgang Pauli, studied the constellations in the sky late at night; a ten-year-old schoolboy from Munich Werner Heisenberg played piano pieces by Schubert; a nine-year-old Bristol boy Paul Dirac responded with stubborn silence to the scolding of grown-ups; and in Baku a three-year-old toddler, Lev Landau, showed the first signs of an independent character.

2

In 1958 when the centenary of Max Planck was celebrated the fifty-year-old Lev Landau was delivering a lecture on the development of quantum ideas to a learned audience in his capacity as a veteran of the quantum revolution. But even he, a veteran, had to start his lecture from a time which he could not himself remember. Still, when he explained why both the action quantum of Planck and the planetary atom of Rutherford were equally and irreparably catastrophic events for classical physics one felt he was speaking about something he personally experienced. That was how many of the contemporary scientists felt about these events in physics—catastrophies!

The best way not to experience a catastrophy in ideas is not to pay attention to it and since only ideas are involved (no houses are destroyed, no storms are raging) one can pretend for a long time that nothing at all has happened.

As Max Born testified, in the years of his youth nobody talked about the quantum catastrophy, even at the most advanced scientific centres of Europe:

> '...As far as I remember, I heard nothing about quanta in Göttingen; I hadn't heard about them in Cambridge either, where I attended the lectures delivered by J. J. Thomson in the spring and summer of 1906, and... was on the experimental course in the Cavendish laboratory.'

Five years later—in that autumn of 1911—when Niels Bohr was also on the experimental course and attended Thomson's lectures in the Cavendish laboratory the other catastrophy—that of the planetary atom—was similarly ignored there. Half a century on the historian Thomas Kuhn asked Bohr directly: 'Was there no one at Cambridge who took the Rutherford atom seriously?' 'No one', said Bohr.

This 'anti-Thomson' atom was disregarded at the Cavendish laboratory to such an extent that it was not even discussed, to say nothing about being criticized. It did not help at all that Manchester was not far away and that Rutherford was an old Cambridge hand. Nothing could overcome this scientific non-recognition. Only half a year after he had come to Cambridge Niels Bohr heard for the first time, and quite accidentally at that, that there was another (non-Thomson) model of the atom—the planetary model.

When the historians asked Niels Bohr: 'Were you the only one who responded well to it?' Bohr said, 'Yes, but you see I did not even respond to it. I simply believed it.' One can well see that from his immediate decision, in March 1912, to switch from Cambridge to Manchester, and from Thomson to Ru-

therford. J. J. let the Dane go without any qualms, not fully appreciating whom he was losing.

But Rutherford, in easily accepting Bohr, did not quite appreciate whom he was acquiring. And, of course, he just could not have imagined that the young doctor from Copenhagen would save his doomed atom.

At the beginning of 1912 Rutherford was still full of indignation at the 'continental physicists who did not worry their heads about the real cause of the thing'. Bohr, being young and unknown, was not of course among the participants of the Solvay Congress and Rutherford had no good reason for being indignant with him personally; on the other hand, he did not have any sound reasons to put his trust in the young man from Copenhagen. He was another 'continental physicist', that was all—and yet Rutherford did somehow manage to appreciate Bohr!

We have no psychological explanation as to how and why he managed to do that... Bohr's highly promising doctoral dissertation on the electron theory of metals was unknown to Rutherford: it remained in Cambridge vainly waiting for publication (Thomson did not find the time to read it even, while Bohr was still officially his student). We could understand it if they were kindred souls. But they were not.

They were markedly different, sons respectively of a New Zealand farmer and of a professor from Copenhagen. One was boisterous, the other was quiet. One made immediate on-the-spot decisions, the other took his time. One was benevolently imperious, the other was shyly reserved. Moreover, they were researchers of different styles. Rutherford used to say: 'Science must be simple if I, a simple man, can successfully work at it.' Bohr could never say that: he

was ready to say that nature was simple—but not the science concerning it! He enjoyed the sophisticated lectures on mathematics delivered by his university professor Thiele; in his old age he explained to the historians that 'it was interesting for a young man who wanted to get to the heart of things'.

Perhaps, that was the similarity between them. It may be that the New Zealander who had proved to the full how deep he could burrow, sensed at the first glance that the Dane was spurred on by the same passion—to burrow to the deepest truth. Rutherford immediately trusted his first impression. That was just his way; we can recall a similar case nine years later when he 'discovered' the young Peter Kapitsa who had come from revolutionary Russia to study in post-war Cambridge and then became his favourite collaborator and student for fourteen years.

Very soon Rutherford was satisfied that he had not been mistaken in his, as yet unvoiced, evaluation of Bohr's capacity. Bohr appeared in Manchester at the end of March 1912 and by the beginning of May had become an expert on the planetary model of the atom.

Everybody was interested in the origin of the electrically charged rays emitted by the radioactive elements—the alpha and beta rays. Rutherford had discovered them; Rutherford had named them; and Rutherford had shown that they were born of the decay of unstable atoms. But scientists could not analyze the details of what happened to such atoms until they had a plausible model of their structure. Now the planetary model—of nucleus and electrons—had appeared, and an answer was needed: what parts of the atom give rise to the alpha and beta rays?

It was clear that the heavy positively-charged alpha particles are emitted by the heavy positively-charged atomic nuclei. But where did the beta rays

come from? Being normal electrons, the beta rays would seem to come from the periphery of the atoms, that is, from the cluster of electrons. They did not, of course, come from the nucleus where there could not be any negative charges!

However, the clarity of such an answer was deceptive—for what then would be the difference between radioactive beta decay and the conventional ionizations of atoms, known from the times of Faraday? (It was Faraday who gave the name ions (from the Latin 'to go') to these atoms which ceased for some time to be neutral.) Atoms could be charged positively by losing negatively-charged particles. Collisions, heating, and chemical reactions readily produced ionization of atoms, and then equally readily restored their neutrality. But radioactivity seemed to have nothing to do with that.

An atom, having emitted a beta particle, was permanently transformed into an atom of another chemical element. No effects—neither high temperatures, nor terrible pressures, nor the most active chemicals —could change anything in the process of radioactive decay. It was during ionization that electrons were certain to be taken from the electron cluster at the periphery of the atoms. But it was anybody's guess in the case of beta decay where they escaped from at such enormous velocities. The planetary model, however, had nothing to suggest but the nucleus and the electron cluster.

It was natural for a young Hungarian radiochemist George de Hevesy (Bohr's friend and contemporary) to put the simplest question to Rutherford: 'Alpha particles come from the nucleus. This is clear. But where from do the beta electrons come?' Of course, Rutherford did not hesitate in his answer. But the reply Hevesy heard was bizarre—the creator of the

planetary model referred him to another authority! 'Ask Bohr...' Rutherford meekly suggested.

When, fifteen years later, an older Hevesy told this incident to the historians they, who knew the ways of Rutherford and the complicated nature of the problem, were no less astonished than the Hungarian himself had been at the time.

> 'Did Rutherford believe Bohr at that point?' Heilbron asked. 'Oh, yes,' replied Hevesy. 'Otherwise, he wouldn't have said "ask Bohr" if he hadn't believed that Bohr actually had a ready answer.'

Yes; Bohr answered that the beta electrons also came from the atomic nucleus! When the nucleus lost a negative electron it increased its positive charge by one and it can be predicted that an atom of the chemical element next to the original element in the Periodic Table should be produced during beta decay. Even that early Bohr understood (though vaguely) that the atomic nucleus was a complex laboratory which could produce particles it did not contain...

The scientists in Manchester now felt that they had acquired a theorist who seemed to surpass Rutherford himself in understanding the secrets of the atom.

Meanwhile Bohr approached the solution of the most difficult of these mysteries: why are the atoms stable if their structure is similar to that of the solar system? if classical laws prohibit the existence of the planetary atom then what are the laws that reprieve that atom?

Almost fifty years later Niels Bohr said in the Rutherford memorial lecture:

'Early in my stay in Manchester in spring of 1912 I became convinced that the electronic constitution of the Rutherford atom was governed throughout by the quantum of action (the Planck constant *h*).'

3

That was what Bohr felt—that the source of one catastrophy was to be found in another!

But he was not timid in tackling both catastrophies and he grasped their very essence. They are beautiful because they create by destroying. Now we can compare them to pinpoint blasting—only one has to understand correctly how they are directed.

To save the planetary model one had to explain the stability of the atom size or the stability of the electrons swarming in the atom. All that everybody else did was to repeat with enthusiasm or disappointment, that each electron at the periphery of the atom has only one fate according to classical laws—to fall into the nucleus. But nobody added that these laws perhaps lose their applicability in the atomic world. Bohr believed in the experimental verification of the planetary model, and thus he dared to put forward this simple but rebellious concept.

Nature itself supported his approach. The tiny atomic worlds are the building blocks which comprise all large and visible objects and the latter prove by their existence that atoms have finite stable dimensions. One had to explain the obvious fact which yet had cosmic significance that the matter comprising the Universe has had an indefinitely long history. No end can be foreseen to this history—the matter did not look like as if it were going to shrink, dwindle,

or condense into a mess consisting of atomic nuclei alone. It was only the stubborn classical laws of physics that indicated such a fate for matter, and they were hardly to blame because they had not been derived from atomic phenomena.

The classical laws did not give any indication of the possible atomic dimensions: these laws allowed electrons (as planets) to revolve around the nucleus at any distance, be it as small as one wished. But clearly there must be among such distances a lower limit, the smallest permissible. It was that distance which should determine the stable size of the actual —allowed to exist—atom.

Classical mechanics could not help one to find this smallest—allowed by nature—radius of the electron swarm. Young Bohr was not asking the classical equations questions they were not made to answer. In the spring of 1912 in Manchester a theoretical premonition came over him (if premonitions can be theoretical); he felt that there should be a profound link between the two 'minima' in nature: the existence of the minimal value for the physical action; and the existence of minimal sizes for the electron swarm in atoms.

In short, Niels Bohr attempted using the quantum approach to explain otherwise classically-inexplicable phenomena. According to the recent prediction of Lorentz he had to become a 'thinker in a remote corner of the world' and find the solution. He spent the summer months in Manchester as a recluse. His winter days in Copenhagen were filled with solitary thought. In fact, his actively searching mind was in a 'remote corner of the world' even as he moved through the noisy university halls.

One should not think that external distractions are the main obstacle to concentrated thought—it is not

so difficult to ignore them. The main obstacle to concentration is the noise of our inner life. The most difficult thing for the mind is to ignore what it itself generates as the unceasing background of its activity... Niels Bohr would always amaze his contemporaries by a capacity for inner concentration.

Once he gave a talk to the physicists in Berlin on his quantum ideas. His friend, the well-known experimenter from Göttingen, James Franck, recalled how Bohr found the answers to complicated questions put by his demanding listeners:

> 'Sometimes he sat there looking practically like an idiot. The face became blank, the limbs hung loosely down, and you wouldn't recognize him even if you knew him. There was absolutely no sign of life. Then suddenly one could see that a glow went through him, a spark caught light and he said: "Now I understand it..." '

That was not a random observation: many years later another physicist Otto Frisch witnessed a similar scene during a seminar in Copenhagen; and Einstein wrote about Bohr's permanent 'hypnotized state'.

But even with his superhuman capacity for concentration it took Bohr almost one year before he could answer ('now I understand') the question, 'Why does the material world huilt from the planetary atoms not disintegrate?'

His thoughts went round in circles before they found the shortcut that led from guesswork to a consistent theory. When it was found, this shortcut, everything was done in less than a month. As often happens, Bohr was helped by chance—something that cannot be planned or prepared beforehand.

Half a year had passed since Bohr returned from Britain to Denmark. One day in early February 1913 he was talking to this former university friend Hans Hansen about his quantum 'torment'. He assured Hansen who worked in spectroscopy that he was close to finding the explanation for 'the properties of matter which depend on the electron system in the atom.' He named the most important properties: the stability of materials, the chemical reactions, the magnetism...

'But how about the spectra?' asked Hansen hopefully. 'How can your theory explain the spectral formulae?'

'The spectral formulae?!' exclaimed Bohr...

Even fifty years later Bohr remembered well Hansen's question and his own bewilderment. Telling the historians about that conversation he smilingly admitted that 'I did not know anything of the spectral formulae'.

Hansen eagerly insisted:

'You must look at the formulae. You'll see the remarkably simple description they give of the spectra.'

'I'll have a look', promised Bohr. Little did he think that in future he would refer to this moment as a turning point in the history of our knowledge of nature.

The same day he found in the literature the well-known (to all but him!) brief formula derived by Balmer for the hydrogen spectrum. Looking at it he at once recognized that it gave a genuine explanation for the stability of the planetary atom.

The Belgian theoretical physicist Leon Rosenfeld who was then a student, assistant, and younger friend of Bohr's recalled:

> 'He told me more than once, "As soon as I saw Balmer's formula, the whole thing was immediately clear to me." '

4

The Russian poet Boris Pasternak wrote: 'Hegel once, without intent (and probably by chance) called the historian foreteller, prophet of past events.' At first the historians found it difficult to understand: why did Bohr himself not think about looking at the atomic spectra?

Indeed, very early on (back in the spring of 1912) he had understood that the structure of the electron system in atoms is governed by the quantum of action. That being so, he immediately should have thought about the radiation quanta; and if he thought about radiation quanta then he was bound to consider the spectra . . .

This obvious line of thinking, of course, presented itself to Bohr. He 'did not know anything' of the spectrum formulae but he did know, of course, that atoms of different elements emit different spectral lines—different series of colour signals. It was a typical metaphor to say that the spectra were 'visiting cards' of atoms. Bohr understood that the structure of atoms was somehow reflected in the spectra, but he thought that they had no direct relationship to the question of stability of atoms. He quite poetically explained to the historians what the obstacle was:

> 'I looked on the spectra as I would on the beautiful patterns on a butterfly's wings: one could enjoy their beauty but could not conceive that their regularity was evidence of fundamental biological laws.'

The Balmer formula described the regularity of the spectral lines of hydrogen: to adapt Bohr's words, nobody thought that this formula provided evidence of fundamental physical laws.

The school teacher Johann Balmer certainly did not imagine that when he published his formula in 1885 (the very year that Bohr was born). The formula was the result of long work and a faith in the regularity of nature: of course there had to be a law to govern the multi-coloured luminescence of hydrogen gas. The scientists already knew the wavelength (or frequencies of the electromagnetic oscillations) for the spectral lines emitted by this lightest gas. Balmer started a numerical exercise and derived his formula without knowing anything about the radiation mechanisms (according to a historical anecdote, he did not think at all about physics). He had simply once boasted that he could find a formula establishing a regular relationship between any given four numbers, and in response a friend gave him the wavelengths of the red, green, blue and violet spectral lines of hydrogen to try and do just that. Balmer pulled off the trick. And for twenty-eight years (up to the beginning of 1913) the brilliant result of his 'number trick' remained uninterpreted by physicists. Nobody could even imagine that behind the simple arithmetics of the Balmer rule the atomic depths yawned invitingly.

In this rule one quantity—*the constant*—was deducted from another quantity—*the variable*—and the value of the variable depended on a series of *integers*. That was all!

The integer 3 substituted into the formula yielded the frequency of electromagnetic oscillations for the red spectral line. The integer 4 yielded the frequency of the green light, the integer 5 yielded the blue line

frequency, the integer 6 yielded the violet line frequency. Other integers yielded frequencies of lines in the ultraviolet spectrum invisible for the human eye.

What was the physical mechanism responsible for the Balmer spectral series? Hundreds of scientists, among them were top-ranking physicists, saw this short formula and could not see what was behind it. Probably Bohr never knew about an event that happened seven years before his memorable meeting with Hansen.

1906. Easter vacation. The Moselle valley in spring. A wayside inn. Two men—one about forty and the younger slightly over twenty—bicycle out from Aachen. They like Moselle wine: the inn-keeper suggests they buy it at wholesale prices. The elder one asks for the guest book; the younger man always remembered what the older man wrote there: 'I shall come for your wine when I have explained the Balmer formula!'

The inn-keeper looks at the two tracks left by the bicycles on the wet soil and speculates when the academic joker will do business with him. But days, weeks, years have passed and the professor from Aachen—the short one with drooping moustache—still does not return to complete the promised deal. It seems he is no longer in Aachen; now, he is a professor in Munich...

That was Arnold Sommerfeld—the one who so exasperated Rutherford at the first Solvay Congress by his disbelief in any models of the atom. Maybe it was this disbelief that prevented him from interpreting the Balmer formula?

Sommerfeld's young companion, who became a well-known theorist in his own right, Peter Debye, told this tale to the historians as an amusing epi-

sode. But the words of Sommerfeld did not sound so merry: one can guess that he was promising to come on 'the Greek calends', that is, never. In other words, even the experienced theorist felt that the Balmer formula presented an impossible task. But the beginner from Denmark simply did not know it existed. Wasn't that ignorance his advantage? (That blessed ignorance which Einstein had in mind when he smilingly recalled how he had discovered something in nature, only because he hadn't known that it was 'impossible' to discover!)

Having seen the Balmer formula, Bohr could not tear his eyes from it—he finally understood.

It was a brilliant example of revelation, a veritable find for psychologists analyzing the creative work of scientists. It confirms once again that revelation comes only to those who seek it. As an inspiration, it is not the precondition for successful work; it is in itself the first success obtained through hard work, when suddenly one starts 'to see far ahead'. The word 'revelation' was used by Bohr himself in his interview with the historians—that was how he felt about what happened. Here is how it happened...

Bohr saw that the Balmer formula essentially described the generation of the light quanta deep in the hydrogen atom. Yes, these were the portions of the electromagnetic energy whose frequencies corresponded to the red, blue, and violet colours, and to higher frequencies invisible to the human eye.

Bohr understood all that from two unremarkable features of the spectral formula—the subtraction sign '—' and the series of the integers 3 ,4, 5, 6... His reasoning had indeed travelled a long hard road in search of the solution to have become so sensitive to the subtlest hints.

The minus sign stood between two quantities—the

larger one (*variable*) and the smaller one (*constant*). The subtraction of the latter from the former gave rise to a portion of light of a definite frequency! That meant that the energy of the atom decreased by the radiated portion of energy. Thus, the variable in the formula represented the energy of the atom *before* the emission of radiation (and therefore was larger), and the constant was the energy of the atom *after* emission (and therefore was smaller).

The emitted portion corresponding to the quantum of the red light would be small, larger when it corresponded to the quantum of green light and yet larger when it corresponded to blue light... That was determined by the integers 3, 4, 5... included in the variable quantity. The higher the integer, the larger the emitted quantum and the higher was the initial energy of the atom, a part of which was then carried away by the emitted quantum. But that had an amazing implication: it indicated that the atom could not have just any energy. The atom's energy varied not continuously but in steps so that it had levels like a house has storeys. Bohr's discovery was that the atom had a stepwise sequency of energy levels.

Each emitted quantum originates from its own level—the red from one (a low one), the green from another (higher), the blue from a third level (a yet higher one)... we can compare it to a running competition in a modern stadium: the starting place for each runner is staggered with respect to the neighbouring ones and it is the only place from which each may start.

But what was the significance of the fact that the second quantity—the atom energy after emission of radiation—remained constant? This residual energy stayed the same whatever quantum was emitted.

What was this then, if not an indication that the atom had a lowest energy level, the 'ground floor' so to say. Thus, the runners we mentioned above have different points of departure but the same finishing line.

This concept was equivalent to the discovery Bohr was striving towards: the atom has the lowest stable state with a nonzero energy. Since it is the revolving electrons that emit radiation in the atom this means that they do not lose the energy of their motion completely—they don't fall into the nucleus. At any rate, the only electron in the hydrogen atom does not approach its nucleus closer than a certain distance. Unknown laws forbid it to do that. Otherwise, the constant in the Balmer formula—the atom energy after emission—would not be finite, it would be replaced with zero, with nothing.

Thus, Bohr suddenly saw that he was on the right track. His letter written to George de Hevesy, his Manchester friend, contains a befuddled phrase with untidy punctuation revealing his excitement:

> 'And the hope and faith in the future (perhaps very soon) enormous and unexpected?? development of our understanding...

Inscrutable are the paths that lead to knowledge... Bohr wrote that letter on February 7, 1913 but just a week earlier in a letter to Rutherford he had stated: 'I do not at all deal with the question of calculation of the frequencies corresponding to the lines in the visible spectrum.'

Looking closer at the coloured patterns on the butterfly wings he saw that they indeed reveal fundamental relationships in the atom. He saw that the radiation of electromagnetic energy quanta and the stability of the planetary atom comprise essentially the same physical problem.

5

Bohr told the historians that two to three days after receiving this advice from Hansen he again met the spectroscopist to explain to him 'how the spectra were born'. Now it was Bohr's turn to say: 'Look, isn't it like this?'—and Hansen's turn to be confused.

> 'He said he did not know' Bohr remembered. 'But I said: "I've got this idea... that you get all these spectral lines out of the difference between the values." But he said he was not sure, so back I went again...'

Apparently, he went back after three weeks when he had written the first part of his famous three-piece work, 'On the Constitution of Atoms and Molecules'. Now he could support his views, not with impassioned words but with sober figures, not with vague images but with exact equations.

(But in telling our good story we shall prefer to use the words and images, and if two or three figures appear here or there that will only be because they are attractively expressive.)

But what did confuse spectroscopist Hansen? could it have been the minus sign underlined by Bohr?

The concept put forward by Bohr is perhaps best represented by a picture of a ladder within the atom; the energy ladder with the lower step just higher than the nucleus and above that steep steps going up into the atomic space where the electrons revolve.

Each step of this ladder is a certain energy level of the atom. The father the step is from the nucleus the higher is the level. Thus, a stone carried up to the tenth storey of a house has a higher energy than

a similar stone taken to the third storey. It is understandable: a larger energy was spent taking up the first stone than on the second, and this energy could not disappear but remained the property of the 'stone-Earth' system.

Incidentally, we see that these laws do not forbid the stone (a macroscopic body governed by classical laws) to be any height above the ground and to drop from any height; there are no steps of energy involved—it can even find itself between floors, it can take up and spend its energy in any portions. In short classical continuity prevails here.

But what Bohr saw in the atom was the quite unclassical ladder of the different allowed energy levels, which are numbered by integers from the Balmer formula.

It now became clear why the atom can emit radiation only in portions—only a stepwise motion is possible along the energy ladder. The atomic spectra—always linear, and never continuous—show that nature does not allow any lingering between steps. Thus we have: in the formula a series of integers, and in the atom a series of energy levels.

The atom is stable at each such energy level until it emits a quantum of light. When it emits this energy portion it drops to a lower stable level in a single jump, without any stop-overs...

That is why Hansen was confused—the picture was too anti-classical.

Of course, one immediately wanted to visualize the energy ladder, at least in the simplest case of the hydrogen atom. Clearly such a ladder can be due only to the single electron which rotates about the nucleus as a planet. And nothing can serve as the energy levels besides the planetary orbits of the electron. Thus one visualized the electron orbits allowed

by nature in the atomic space; each orbit corresponded to a certain energy of the electron, and hence, of the atom. Thus, we now have: in the formula a series of integers, and in the atom a series of electron orbits.

According to this picture, each electron revolving around the nucleus along its own orbit does not emit light—this is directly against classical laws. Only when the electron drops from some higher orbit down to the lowest of the allowed orbits, only then does the atom emit a quantum of light corresponding to the depth of the fall. The further the starting orbit from the nucleus, then: the larger the emitted quantum, the higher the frequency of its electromagnetic oscillations, and the closer the corresponding spectral line to the violet end of the atomic spectrum...

That was another cause of Hansen's confusion: this theory gave an unthinkable answer to the simple question 'What is the relationship between the frequency of the emitted light, and the frequency of rotation of the electron around the nucleus?' The answer was even simpler: 'None!' Indeed, as long as the orbital motion is stable—i.e. the energy is not changed—the radiation process is quite independent of the frequency with which the electron travels around the nucleus.

But it was long known in classical physics that the frequency of radiation emitted by a body is equal to the frequency of the motion of charges in it. That was a well-known fact. It was not questioned either by Maxwell or Hertz in the last century, or by Lorentz, or Planck in this century. It is no wonder that young Hansen who had not even yet gained his doctor's degree was bewildered (and he was preparing for a degree in spectroscopy, so the new theory meant he had to learn much over again).

Maybe, Albert Einstein was the only scientist who questioned the traditional concept. However, he never had voiced any doubts until the Bohr theory appeared. When in September of 1913 Hevesy had a chance to tell him about the experimental verification of the Bohr concept, Einstein exclaimed:

> 'Then the frequency of the light does not depend at all on the frequency of the electron? ... And this is an enormous achievement. In that case the theory of Bohr *must* be right.'

On another occasion Hevesy said: 'The big eyes of Einstein grew still bigger and he told me: "Then it is one of the greatest discoveries".' But the most interesting revelation is that Einstein added an unexpected confession that 'he had once had similar ideas himself but he did not dare to publish them'.

An Einstein who 'did not dare'—who can imagine the author of the theory of relativity and the quantum theory of light in such a role?! But he said so himself. Maybe, he had lost his daring with the years? But no, when he said that he was only thirty-four and his mind was full of the supremely daring theory of gravity. The reason was clearly different. It is an interesting subject for speculation.

He would have had the courage if he had had confidence in the inescapable consequences of the new physical concepts for the atom. Though they were quite vague at the time his powerful insight immediately prompted him to foresee that trend of events: but the 'similar ideas' he once had ran quite contrary to his own philosophy of nature.

This was founded on an absolute belief that nature was governed by laws of definite determinacy, and any indeterminacy allowing absolutely random

events was alien to it. That was what Einstein had in common with the creators of classical physics of which he was the final great builder.

After some years he formulated his classical belief in the well-known joke: 'I do not believe that God plays dice!' We shall discuss this formula of his in connection with his refutation of another philosophy of nature, the one in which the laws of probability govern the submicroscopic world. An unresolved controversy between him and Niels Bohr on that subject went on for several decades—imagine this unceasing drama of ideas in the spiritual life of Einstein.

But did not this drama begin before, in those early years when he 'did not dare' to follow through the ideas that young Bohr later bravely developed into a complete theory? At that time his argument was with himself—an internal dialogue between Einstein the physicist and Einstein the philosopher.

Such a controversy could arise quite simply: when the physicist Einstein wanted to explain that there was no direct cause of relationship between the frequency of rotation of the electron and the frequency of the emitted light, the philosopher Einstein stopped his 'twin', and did not allow him to dare. It was as though the philosopher was afraid that if today he dropped the definite relationship in one special case, then tomorrow he would have to refute classical determinacy altogether. But no, that would be too much! There must be something wrong here...

Nevertheless Einstein the physicist rejoiced to see ideas close to his own embodied in the theory of the atom: 'This is an enormous achievement...!' In old age Einstein would say of the Bohr theory, 'It still seems a miracle to me. This is the highest musicality of mind.'

6

Einstein did not explain what he meant by the musicality of mind—there can hardly be any prosaic explanation of it. (Incidentally, Bohr was no musician, in contrast to Einstein who was an accomplished violinist, and Planck who was a skilled pianist.) But this was something different: the harmony of thought and the harmonious features he saw in the very structure of the quantum theory of the atom.

Looking at this theory one could not help remembering 'the music of the spheres' of the Pythagoreans, the predetermined harmony of motion of the celestial bodies, and the harmony of the integer relations in the structure of the world.

The ladder of the atomic energy levels in a picture of the quantum model looked like the liner of the musical staff. One had just to draw in black points for the electrons jumping down this staff like the notes of music and one had a graphic metaphor: nature played the spectral music of the multicoloured diversity of the world. (The words 'spectral music' belong to Sommerfeld.)

Niels Bohr, who only recently had known nothing of the Balmer formula, was understandably keen to analyze all such formulae accumulated in optics. Following the Swiss teacher, the scientists used this formula to describe the sequence of frequencies in other series of spectral lines, and not only those of the hydrogen atom. The most recent and most general formula derived by Walther Ritz gave such a description for any spectral series.

One can easily imagine the increasing delight of the quiet Dane as he repeatedly obtained the confirmation for his theory: the spectral lines were always

found as a combination of two quantities with a minus sign between them!

The Ritz formula had a new feature which the Balmer formula lacked: both quantities—the atom energies after and before the emission—proved to be variable and both dependent on the series of integers. But Bohr expected that. He could easily explain this complication. It meant that the electrons emitting the quanta did not have to drop each and every time to the lowest step of the energy ladder. The electron from an upper level could drop to any intermediate orbit allowed by nature, and rotate stably along it. Therefore the atom energy after the emission of quanta could also be variable.

One could jump along the ladder of the energy levels in any fashion, missing one, two, three steps or simply all the intermediate steps. The length of the jump only determined the magnitude of the emitted quantum, that is, the colour of the spectral line. It was not accidental that the Ritz formula was known as the 'combination principle' in spectroscopy; the calculation of the frequencies in the atomic spectra was reduced to the combination of integers. Until the Bohr theory emerged nobody had known the significance of these integers: a network of numbered electron orbits? ... the numbers of steps on the energy ladder? ... a discrete sequence of the stable (stationary) states of the atom? ... Such concepts and images were unthinkable for a classical physicist spectroscopists working in Göttingen. This highly talented scientist discovered his combination principle at the age of thirty (1908). However, like Balmer he could not appreciate the significance of his forbrought up on the age-old cult of the continuity in physical processes.

Walther Ritz belonged to the prominent group of

mula. We should understand that this was not their fault: an elderly Balmer died in 1898, two years before the concept of the radiation quanta emerged; while Ritz died young in 1909—two years before the planetary model of the atom made its appearance. It could only be an encounter between this concept and this model in one mind that would reveal the mechanism of the instrument on which nature plays the spectral music. Or, to use another more mundane comparison, to reveal the method of nature's bookkeeping for the electromagnetic energy in atoms. At the same time the explanation of the atomic spectra saved the planetary model from the menace of the instability of the material world.

The solution of the stability problem promised by Rutherford in 1911 now logically followed from the two postulates proclaimed by Bohr. Actually, we already know them, since we have been talking about them here all the time: according to the first postulate, the atom has a discrete sequence of stationary states; according to the second the atom emits energy quanta during transitions between these stationary states.

One immediately sees that one state among the series of possible stationary states differs from all others —it has the lowest energy. The atom can go no lower, it can lose no energy in this state—therefore it can remain in it as long as desired! This means that the first allowed electron orbit in the atom is the closest to the nucleus, and the electron cannot rotate closer to the nucleus as there is no integer between 0 and 1.

Over twenty years later when physicists celebrated Bohr's 50th birthday somebody parodied the English nursery rhyme 'This is the house that Jack built' in a song, 'This is the atom that Bohr built'. His atom

looked like the soaring sky-scrapers dreamed up by Le Corbusier: the upper stories were built in a normal way but there is no ground floor, the whole building standing on concrete columns and looking as if it were suspended in the air. The first electron orbit in the Bohr atom lies high above the nucleus and the space between the nucleus and this orbit is 'out of bounds' for the electron—it is not allowed to go there. But it can stay indefinitely in the first orbit.

Hence, this state of the atom is the most stable one. (Bohr called it the *ground* state.) It now became reasonable to ask what size the atom was. Any electron orbit could determine the possible boundaries of the atom as if it could swell, but the inviolable limit was that defined by the electron orbit closest to the nucleus. Its radius should be regarded as the normal size of the atom.

Bohr managed to calculate it; it was of the order of 10^{-8} cm.

A hundred millionth of a centimetre is called one angström unit. Physicists and chemists were already acquainted with this unit for the hydrogen atom—they had found it from indirect experimental data. And now it could be derived directly from the atomic structure! This fact greatly impressed Bohr's contemporaries.

Another number that made a great impression was 109 000: that was the value obtained by Bohr for the spectroscopic constant appearing in all spectral formulae, and known as the Rydberg constant. Its experimentally measured value turned out to be 109 675. The agreement between theoretical prediction and the experimental data was astounding.

The quantum concept of the atom clealry deserved the confidence of the scientists. And not only their confidence but also... Yet is there another form of

approval for a scientific theory? George de Hevesy from Rutherford's laboratory, after reading Bohr's fundamental paper, immediately wrote to him: 'Your paper has been a great source of pleasure for me'. And he tried to explain why:

> 'A thinking mind does not feel happy until it can connect together the individual facts it has observed... It is this "intellectual unhappiness" which induces us to think, to make science.'

These words reveal a remarkable intellectual feature common to the veterans of the quantum revolution in physics. The memoirs, letters and taped interviews vividly demonstrate that they were permanently motivated by the creative urge to get rid of an 'intellectual unhappiness', which became more acute the more they felt that their investigations should reveal fundamental relationships between yet unconnected facts...

Hevesy said that when he studied the quantum theory of the atom he could be regarded as a truly happy man.

Chapter Four

A Road in Darkness

1

It is quite logical to say that any scientific discovery decreases the region of the unknown. But it is no less logical to state that this region grows with the discovery, and it is the discovery itself that increases it: when a man goes up in the mountains the horizon widens but the lands beyond the horizon extend even more.

Louis de Broglie gave a good description of this well-known result of scientific progress:

'The beautiful fresco painted by Puvis de Chavannes in a large auditorium of the Sorbonne shows a wide clearing in the woods filled with human figures: the figures, stylized in the usual manner of this painter, symbolize humanity enjoying the highest spiritual values —literature, science and art; but the clearing is surrounded by dark woods which symbolically indicate that the mysteries of the world surround us on all sides despite the brilliant achievements of the human mind.

'Yes, we are at the centre of a great dark wood. Little by little we clear a small plot of land around ourselves. Thanks to the advances of science, we continuously extend its borders at an even increasing rate. However, all the time we have before us this mysterious edge of the woods—the impenetrable and infinite woods of the Unknown.'

When de Broglie described his feelings in this way, in his lecture 'Pathways of Science' in February 1955, he was close to seventy years old. He had known the joy and sorrow of profound conjectures, of dramatic crises, and of years of fruitless work. Among the men and women of letters and science attending to his lecture perhaps only a few knew about de Broglie's drama of ideas (which just at that moment was repeating itself), but everybody attentively listened to the veteran of the quantum revolution. Some of them intuitively felt and others were quite aware that he had every right to speak in such lofty terms about the 'mysterious edge of the woods of the Unknown': for he himself had helped to clear a part of this edge.

Compare the following dry words of another au-

thor with the scientist's flowery style: 'Science always proves wrong. It never solves a problem without posing dozens of new ones.' This is a typically Shavian statement: since the conventional wisdom is that science always operates with proven truths, he must convince us of the opposite, that it is always wrong.

He was thinking about science at the moment of its triumph, at the moment of discovery: then it is veritably disarmed before the new questions which it has itself revealed by thinning the edge of woods of the Unknown. It as yet has no answers to these new questions which would never have appeared before the discovery was made: once again it is 'proved wrong' just at the height of its success. And the larger the discovery, the more 'wrong' science proves to be, i.e. the more new questions it raises. This had happened with the discoveries of Planck, Einstein and Rutherford; now it happened with the quantum theory of the atom, as well.

After Hansen's helpless 'I don't know', the first and the most significant questions were put by Rutherford. It was natural that his questions were the first: Bohr, of course, sent him his manuscript to report that the planetary model was reprieved. But that Rutherford's questions were also the most penetrating may seem strange; he never regarded himself as one of the pure theorists and even liked to joke at their expense. He enjoyed saying that they had only to 'get their tails up' when 'we, experimentalists, pulled them down again.'.

However, the planetary model was his theoretical baby and, of course, he made numerous efforts to save it; his critical insight had been sharpened greatly by lack of success. He immediately replied after receiving the manuscript, and there was one comment in his letter that deeply impressed Bohr:

> 'There appears to me one grave difficulty in your hypothesis, which I have no doubt you fully realize, namely, how does an electron decide what frequency it is going to vibrate at when it passes from one stationary state to the other? It seems to me that you would have to assume that the electron knows beforehand where it is going to stop'.

Thus, for the first time (and in a private communication) there appeared the significant suspicion: does not the quantum theory provide a tiny particle, the electron, with 'freedom of will'?

Fifteen years later when the 'Sturm und Drang' period reached its climax, and philosophical controversies around the quantum ideas grew increasingly violent, the cautious question posed by the great physicist had attained the proportions of a grave charge under the pens of scientific polemicists. 'The free will' of the electron would be the 'bête noire' of everybody who was exasperated by the new physical thinking. And the cause of their exasperation would be the questioning of the classical absolute determinacy which science had assumed to be all-powerful in nature. Incidentally, the imaginary 'free will' of the electron was to be joyfully (though mistakenly) taken up by the mysticists, who did not want any determinacy—neither absolute, nor probabilistic—and were partial only to indeterminate and improbable imaginary events.

However, this was not the fault of Rutherford since his letter to Bohr was published only in 1939, more than twenty-five years after it had been written... Rutherford had put a purely physical question; and he was right—Bohr's theory had some very strange implications. Assume that the electron has a choice

of transitions and performs one of them—from an outer orbit to an inner one. The depth of fall determines the energy of the emitted quantum, its frequency or colour. That means that at the moment a jump starts everything is determined by its end, the distance of the jump. The radiation frequency cannot change during this process; any quantum is of one colour or monochromatic, in the language of physics. In other words, the electron has to choose one of the lower orbits beforehand, to calculate 'where it is going to stop'. This looks as if it had a free choice—as if it could decide its quantum fate beforehand!

Rutherford was right in his confidence that Bohr 'fully realized' this obstacle in understanding his concepts. But what could Bohr do? He found himself in exactly the same situation as Rutherford two years earlier when the latter had put forward the classically-impossible planetary model of the atom.

In 1911 the New Zealander hoped for a future solution to the stability problem for his model. Now it had been found. But in 1913 the Dane, in his turn, could hope only for a future justification of his concepts which had saved the planetary model, since they also proved themselves to be inconsistent with classical common sense.

However, even then Bohr made some attempts at self-justification...

2

He had to follow his manuscript to Manchester to face the criticism of Rutherford. When, many years later, he was speaking about the attitude of Rutherford towards his concepts the first things he recalled exactly repeated the words from Rutherford's letter;

in spring 1961, during Bohr's last visit to Moscow, he told physicists there:

> 'Rutherford did not say that it was stupid, but he just couldn't understand how the electron starting a transition from one orbit to another learned which quantum it had to emit...'

But it is extremely interesting (and it is worthwhile recalling Rutherford's question here) that, as Bohr added: 'I told him it was like the "branching ratio" in radioactive decay, but he wasn't convinced.'

The young Bohr thought that he had found a psychologically apt and precise scientific argument to disarm Rutherford; the branching ratio—a strange effect in the radioactivity phenomena—was well known to Rutherford. Some of the radioactive elements decayed in two ways; two branches of radioactive transformations could grow from one point. For instance, in the uranium family two branches developed from the radium-C: a small proportion of the atoms of this element underwent alpha decay, while the greater proportion underwent beta decay, on average three atoms out of 10 000 were transformed into tellurium and 9997 into polonium. In other words, nature presented each atom of radium-C with a choice of two fates. It appeared as if the nucleus, under exactly similar conditions, could decide beforehand what it would emit—an alpha particle, or a beta particle. Other cases of this effect were known, with another branching ratio between the alpha component and the beta component. Rutherford knew this effect and did not show surprise at it.

But indeed it was extremely similar to the choice the atom faced before emitting light—which quantum to emit, a red one or a blue one? Or, in other words,

which orbit the electron should leave and which orbit it should enter. In addition, it was clear from the different intensities of the spectral lines that some quanta are emitted more frequently than others, just as the atomic nuclei prefer one type of radioactive decay to another at the branching points in the series of radioactive transformations.

Thus, there was a good parallel to justify the new theory. But one fault cannot be justified by the fact that a similar one has already been made. Parallels cannot explain the facts, they only illustrate them. By doubling something unclear they just make it more visible, to the initial unknown quantity they add a similar new one.

But it was precisely the similarity that was most important for Bohr. A common feature is manifested in different atomic phenomena, in the behaviour of the nucleus and in the behaviour of the electron. The repetition contradicted the classical concept of 'the same behaviour under the same conditions' and revealed some behaviour under the same conditions' and revealed some mysterious regularity in the classically unlawful events. Though it was 'mysterious' it was still regular...

It was a psychologically successful move on Bohr's part: the parallel to radioactivity, without eliminating Rutherford's question, reconciled him to the strange features of the quantum concept that saved his planetary model—at least, in one respect. But there was something else involved...

3

The Dane had started his theoretical work in Manchester almost a year earlier with the approval of

Rutherford. Later, Rutherford expressed his support in correspondence. But now, in March 1913, having read the finished work of Bohr, Rutherford had to recommend it for publication in the *Philosophical Magazine*; he felt, however, that the very foundations of this work could be questioned. Rutherford was concerned with something more serious than the 'free will' of the electron; in his letter Bohr read the following:

> 'Your ideas as to the mode of origin of spectra in hydrogen are very ingenious and seem to work out well; but the mixture of Planck's ideas with the old mechanics makes it very difficult to form a physical idea of what is the basis of it all.'

Indeed, the electron-planets revolved around the nucleus-sun according to the laws of the classical mechanics, but between these orbits they emitted light according to the quantum law of Planck. This picture combined irreconcilable concepts—the classical continuity and the quantum discreteness.

A friend of Rutherford's, William Bragg, joked later that Bohr's theory suggested to physicists that they use the classical laws on Mondays, Wednesdays and Fridays, and the quantum laws on Tuesdays. Thursdays, and Saturdays. It was all correct mathematically but the physical picture was ambiguous.

The first quantum concept of the atom was very vulnerable!

The venerable professor of spectroscopy in Göttingen, Carl Runge, took a dim view of the new theory: 'Now the literature on spectroscopy will be permanently contaminated with terrible things.' (In one respect he was right—the change was indeed permanent.) He fulminated against Bohr: 'This fellow is

definitely mad!' Runge's son-in-law, the prominent mathematician Richard Courant, who was friendly with Bohr, did not dare to tell him what his peeved father-in-law was saying; half a century later the historians heard much more from the now elderly Courant than in his time young Bohr had heard from his contemporary, the young Courant.

When the great classicist, Lord Rayleigh, was asked at a scientific meeting in Britain to comment on the quantum theory of the atom he just smiled:

> 'In my young days I took many views very strongly and among them that a man who has passed his sixtieth year ought not to express himself about modern ideas. Although I must confess that today I don't take this view quite so strongly, I keep to it firmly enough not to take part in this discussion!'

He was just over seventy at the time. Despite his charmingly disarming refusal Rayleigh still took part in the discussion without noticing it himself. He said among other things: 'It's hard for me to accept all this as a true picture of what really happens in nature.'

Incidentally, one did not need to be over sixty to refute the 'newest concepts'. Indeed, one even did not have to be older than their author: Otto Stern was three years younger than Bohr, but in 1913 when he was just twenty-five he vowed that he would drop physics 'if this nonsense proves to be true' (that's what he later told his student Otto Frisch).

In the autumn days of 1913 when the elderly Rayleigh was not able to say much about the quantum theory, at another scientific meeting—now in Switzerland—thirty-four year old Max von Laue, a former assistant of Planck, barked: 'Rubbish! .. The electron

at the orbit must emit radiation!' Luckily, Einstein who happened to take part in the same meeting in Zurich made a similarly terse retort: 'No, this is remarkable! There is something behind it...' And Einstein was also only thirty-four. Thus we see that age really does not matter—something else is important...

When the Bohr theory appeared Rutherford did not praise it much more than to say that it was 'ingenious'. But one is sure that he, like Einstein, thought 'there is something behind it'. Though Bohr who had rushed to Manchester could not eliminate any of Rutherford's doubts, after many hours of discussions, Rutherford still recommended the paper for publication.

This just showed his broad-mindedness and tolerance. It is the more surprising that Rutherford never supported the publication of findings he thought were not reliable enough. Of course, he felt that there was a higher truth which had yet to be revealed behind the Bohr postulates—and he knew from his experience that science could not survive without broad-mindedness and tolerance!

4

In that first public discussion of the quantum concept of the atom, during which Rayleigh had aired his views on men over sixty, Lorentz who had himself just turned sixty also took part. 'How can Bohr's atom be explained', Lorentz asked, 'from the point of view of mechanics?' Many thought it a biting question, though that was not at all typical of Lorentz' character.

Bohr understood that Lorentz' question was as

plainly straightforward as that of Rutherford's: it concerned the most important subject, the logical relationship between the quantum postulates and the classical mechanics. A mechanical interpretation was needed for two phenomena, the existence of the discrete sequence of the allowed electron orbits in the atom, and the jumps of electrons from one orbit to another .What could Bohr answer?

The quantum postulates were not derived from the classical laws; otherwise, they would not be, first, postulates and, second, quantum ones. Though a web of allowed orbits and the jumps of electrons emitting quanta are both equally strange to a classical understanding of nature there is a marked difference in how well our imagination can cope with these two concepts. The former is generally quite conceivable but the latter is virtually impossible to visualize. It does not matter in the first case that the orbits are unseen: our thoughts can easily trace them, one by one, in the atomic space like the paths of the planets in the space of the solar system. This is a fixed geometric image. But a jump involves motion, and we must visualize the process of electron motion between the orbits. This would appear very easy, but unfortunately, these are quantum jumps. And what seemed simple becomes impossible. Our imagination boggles at it.

The quantum jumps have a beginning and an end, but the middle—the jump itself—is indescribable in terms of conventional mechanics. To make a mechanical description we would have to divide the quantum jump into smaller and smaller jumps to trace the process—and that is forbidden by the definition of the quantum jump: it would mean dividing the quantum into parts, into smaller quanta of radiation, and involve the breakdown of the very concept of in-

divisible quanta of energy. The atom where such division was possible would emit light continuously; its spectrum would be continuous, rather than consisting of lines.

For the first time physics had to deal with physical events that had no mechanical history. Just as classical mechanics, our imagination refuses to serve our thought. It is not surprising that many years after Bohr's theory had been born, Landau said that the quantum ideas proved to be even more 'wild' than those of the relativity theory.

So what could Niels Bohr say in answer to Lorentz in 1913? There were no logical relationships between his theory and classical mechanics; he could only say he was sure that '...since we cannot do without the quantum theory we need some mechanism involving discreteness and jumps!' Nothing could be said against that.

Still, there was a point where a profound relationship between the classical laws and the quantum features of the atom structure could be traced. This relationship, which Bohr then called the 'similarity considerations', was later formulated in his famous 'correspondence' principle.

The ladder of the stable energy levels in the atom had one highly interesting and very noticeable feature: the further from the nucleus the smaller were the steps of this ladder. If a man stands on the ground and looks up at a Mexican pyramid he sees how its steps vanish towards the top. But this is an optical illusion due to the perspective; all the steps, in fact, are of the same height. But in the atom the steps are actually different—the decrease of step height with the increase in distance from the atomic nucleus is not illusory; it is real as shown by formulae and experimental spectra.

The difference between the neighbouring allowed energy levels becomes increasingly less noticeable, and the electron orbits grow increasingly close to each other. The jumps from one energy level to another—from one electron orbit to another—become increasingly shorter. In the spectra reflecting these jumps the emitted lines draw closer. The line spectrum appears similar to a continuous spectrum, as if the atom were emitting continuously all the quanta in the light frequency range. Discreteness is gradually transformed into continuity; the quantum laws give way bit by bit to the classical laws. The physical picture gradually changes from that of the submicroscopic world into that of the macroscopic world, as nature brilliantly demonstrates its physical unity.

As should be expected, there are no border posts in nature declaring 'Up to here the domain of Galileo, Newton and Kepler, and from here the domain of Planck, Einstein and Bohr.' There is no 'iron curtain' between the atomic world and the visible world; otherwise, we who are speculating at our leisure about laws of nature, would not have the honour ourselves to be complex structures of atoms (and would not be able to speculate about them).

Bohr would have had to derive the 'similarity considerations' or the correspondence principle from his theory if only to satisfy his own philosophical needs: however, he derived this principle (from formulae and experimental data) for other, more mundane, reasons. Much had to be explained in the behaviour of atoms as emitters of the quanta, to speak nothing about the chemical features and many other aspects. An encouraging sign was that the classical laws were increasingly applicable to a description of the electron motion that with increasing distance from the nucleus took place at the atomic periphery. It was

suggested that the required quantum formulae could be derived by their similarity—'correspondence'—to well-known classical laws.

Perhaps, it was the first time that a theory was developed in such an illogical way in physics, reported to be a highly logical science. One can barely believe the following admission of one of the geniuses of the quantum physics, Werner Heisenberg:

> 'We aimed our efforts not so much at deriving the correct mathematical relations as at making guesses about them, proceeding from their similarity to the formulas of the classical theory.'

And they made correct guesses!

Arnold Sommerfeld was delighted with the 'magic force of the correspondence principle'—the quantum model of the atom even in its initial, far from perfect, form gave so many good answers (backed up by the experimental results). Even though the questions of Rutherford, Bragg, Rayleigh, Lorentz and others could not be answered one yet felt that its foundations (still unfathomed) were essentially correct. But what were they?

Sommerfeld wrote to Einstein at the beginning of the twenties: 'Everything is going well but the basic foundations remain unclear.' Echoing him, Max Born said that the deep causes underlying the foundations of Bohr's theory were 'quite mysterious'.

Einstein himself did not question its validity but he wondered in his typically, mildly ironical fashion: 'If I only knew which nuts and bolts God uses here!'

Many physicists hoped at the end of the first and the beginning of the second decades of the 20th century that it would be precisely Einstein who would wrench the answer from nature as to which nuts and

bolts it employed to make up the atomic emitter of the quanta and, more generally, to hold together the submicroscopic world. A letter written by Sommerfeld to Einstein reflects his trust in the keen insight of the man who had created the quantum theory of light:

> 'You are thinking about the fundamental problems of the light quanta. But I, who do not feel strongly enough about it, am satisfied with an understanding of the details of the quantum miracles in the spectra... I cannot suggest anything to account for their physical meaning.'

He wrote also: 'I can only help to develop the techniques of the quanta. You must build their philosophy.'

However, it was not Einstein who was destined to build it. On the contrary, his fate was to become its life-long 'enemy'—a tireless, inventive, tenacious but ultimately futile opponent. This fate seems even more dramatic when one recalls that Einstein was present at the cradle of the 'philosophy of quanta'; in fact, it was he who had placed in this cradle the baby who would grow stronger and stronger.

The baby was a centaur—it combined the properties of both particles and waves.

5

The concept of these submicroscopic centaurs—the particle-waves—was not needed at all for saving the planetary model of the atom from instability. Of course, the jumps along the energy ladder of the atom are accompanied by the emission or absorption of

the quanta of light. But it was irrelevant for the theory of the atom what spatial form each quantum had, whether it was a distinct corpuscle of radiation or a sequence of electromagnetic waves. It was irrelevant to such an extent that Niels Bohr allowed himself to deny the reality of the particles of light suggested by Einstein and recognized only the quanta suggested by Planck—the portions in which nature measures up the electromagnetic energy of radiation. It was quite understandable: at first Bohr was concerned only with the way the radiation energy was delivered. The emitting atom delivered the quanta of radiation as the difference between two energy levels. That was all. The strange features of the quanta of light emitted by the atom were not interesting to Bohr at the time.

We can recognize here a familiar process in the history of knowledge—how science had to limit its scope of investigation to produce a success. Recall once again the words of Plato's Thymeus: 'If we want to study astronomy we need not be interested in the celestial bodies.' But, of course, this limitation is kept up only for so long and no longer. The former President of the USSR Academy of Sciences Sergei Vavilov expounded this idea of Plato's in his book on Newton:

> 'At many stages in its history science closed its eyes for the time being to groups of factors and entire ranges of phenomena which complicated the task.'

The apparent strangeness of the very nature of quanta was reflected in the puzzling duality of their behaviour: they appeared now as corpuscles and now as waves...

Strikingly, it was very long ago that physicists no-

ticed this duality of light. More than two centuries before in 1756 Lomonosov summarized the different opinions on the motion of the 'finest and intangible matter of light':

> 'The first motion could be flowing or passing, as Gassendi and Newton think, in which the ether (the matter of light as designated by the ancient and many modern authors) travels from the Sun and from other great and small luminous bodies to all sides continuously like a river. The second motion can be undulation in the ether when, according to Descartes and Huygens, very small and frequent waves are propagated on all sides of the Sun...'

Here Gassendi and Newton are mentioned as the adherents of the theory of light according to which it is a flux of particles; Descartes and Huygens are represented as the supporters of the wave theory according to which light is a flux of waves. Interestingly, even at that time Lomonosov had to recognize the reality of both types of behaviour of light—he said of the light matter that both these 'possible motions' indeed could be found in it. He entrusted the future with verifying these theories: particles or waves? 'What indeed exists and what does not later will be shown!'

The wave theory could not explain the simplest feature of light, its propagation in straight lines, for a very long time, for about 150 years according to Vavilov. Perhaps this is why throughout the 18th century the corpuscular theory was dominant in physics, though a few disagreed. It was precisely this feature it could explain very simply—how else could the particles of light travel in the emptiness if not along the straight lines?!

It was this propagation of light along straight lines that prevented the corpuscular theory from describing another effect: the capacity of light to go around obstacles, known as diffraction. Owing to diffraction shadows never have absolutely distinct boundaries. If light consists of waves this can be easily accounted for: waves blur the borders of the shadows as they can go round the edges of objects but the particles flowing along straight lines cannot do that. Diffraction provided evidence against the corpuscular theory, and contributed to the triumph of the wave theory.

Another puzzling effect was interference. Newton himself used to demonstrate it: 'If a convex plate is put on a flat plate the dark and light rings can be seen in uniform light.' He explained that these rings were a result of superposition of 'transmitted and reflected light'. But it was difficult to explain how darkness appeared where two beams of light met (were superposed) if they were the fluxes of particles: in that case the luminosity should increase.

But the interference effect was not difficult to explain at all for the wave theory. The waves could be mutually intensified when the crests were superposed, and they could be mutually cancelled out when a crest of one wave met the depression of the other wave. The series of light and dark rings could be naturally treated as a wave picture.

Incidentally, it was quite irrelevant for the theory of waves what was the material in which the waves constituted light—whether it was the imaginary ether or the more real forces of the electromagnetic field. Further on in our good story we shall meet other waves which can also interfere with each other. The following words of the English physicist Rudolf Peierls can be seen as a development of Plato's thought:

> '. . .To understand the interference one need not know the nature of the wave. It is sufficient to know that there is a quantity which is oscillating. . .'

Oscillations of the opposite signs, that is oscillations in the opposite directions, can cancel out each other, while oscillations of the same sign can intensify each other—that is all there is to the interference effect. It gave the strongest support to the wave theory of light; in addition, the wave theory managed to give a perfect explanation for the straight-line propagation of the light rays. It is no wonder then that throughout almost the entire 19th century the wave theory was dominant. The corpuscular theory was a thing of the past.

But the 20th century came and brought Planck's quanta and the light particles of Einstein's... Has history turned full circle? As if he had foreseen that this would happen once more, Newton's teacher, Isaac Barrow, said:

> 'Both concepts of light meet equal difficulties. Therefore I tend to think that light can be produced by both types of motion—by a flow of particles and by continuous pulses. Maybe some actions should be ascribed to the one type of motion and other actions to the other type.'

One might think that the teacher was more farsighted than his great student; but no, Vavilov reports the following words of Newton:

> 'If we assume that light consists of small particles ejected in all directions by the luminous body then these particles... must excite

> oscillations in the ether as inevitably as a stone dropped into water...'

We can see that Lomonosov was wrong in representing Newton as a supporter of the corpuscular theory and an enemy of the wave theory of light. Newton even suggested a constructive way of reconciling the incompatible concepts of the particle and wave! Perhaps this suggestion would have been suitable had it not distinguished between the matter of the particles and the matter of the waves: but it implied that the luminous body emitted one thing while something else was undulating in space. Nevertheless, one can see here a fascinating rapport between the great scientists across centuries—among all the geniuses of classical physics Newton would probably have been the one least perturbed by, and the most sympathetic to, Einstein's concept of the particle-waves... (Until he discovered, as Einstein did, what irreparable damage this concept did to classical physics).

To be accurate, Einstein did not use the term 'particle-wave' when he rediscovered the corpuscles of light forgotten during the 19th century. But in his strange concept each quantum had a wave property—the oscillation frequency, and a particle property—spatial boundaries.

Human imagination could not cope with this ambiguous concept; human logic also found it difficult. Time passed but it did not become any easier:

> 'Now we have two theories of light: both are necessary and—it should be admitted today—both exist without any logical interrelationship despite twenty years of colossal efforts by the physical theorists.'

Einstein said that in 1924. And he added, as if in answer to the unasked question 'Why had the scientists not suggested something more suitable in these two decades?'

> 'The quantum theory of light paved the way for the theory of the atom developed by Bohr, and explained so many facts that it must be true to a considerable extent.'

Bohr should have welcomed these words, if anybody did! But in the same year, 1924, he said to the young Werner Heisenberg, not without exasperation,

> 'Even if Einstein had sent me a cable telling me that he had the final proof of the reality of the light particles, even then this message, sent over the wireless, could have reached me only via the electromagnetic waves comprising radiation!'

It is a brilliantly witty statement but the duality of the radiation quanta was not an invention of Einstein's. The history of science can be read as a subtle psychological novel. Bohr had not noticed that his witty retort only emphasized the inevitability of accepting the duality of radiation he was so firmly against: none other but he had admitted that the quanta could be finally proved to be real particles without denying their reality as waves!

This unwitting admission was a good sign—his search for truth was soon to lead him to a full realization of Einstein's rightness. As a hero in a well-written novel, he had psychologically prepared himself for that. And the consequences for physics were extremely significant...

The dénouement came in July of next year, 1925. A series of experiments performed by German exper-

imenters made Bohr abandon hope of eliminating the dual nature of the quanta. He was finally satisfied that it was nature that furnished us with the concept of particle-waves, rather than Einstein himself. Bohr wrote the following prophetic words at this hour of heightened awareness:

> 'In this situation one should be ready for a resolute restructuring of the concepts on which the description of nature has hitherto been based.'

There was another event in physics that had a significant effect on Bohr at the time. It took place in Paris.

6

In the late autumn of 1924 a thesis entitled 'Studies in the Theory of Quanta' was submitted at the Sorbonne. To outsiders the man who wrote the thesis was much more interesting than its subject: everybody knew that thirty-two year old Louis de Broglie was a younger brother of the well-known physicist Duke Maurice de Broglie and held the even more elevated title of prince. Moreover, he had already distinguished himself by obtaining degrees in literature and history in his early youth, before the recent Great War. And now he became a doctor in natural sciences.

Though it is just idle curiosity, one cannot help noting that the de Broglies belonged to the Piedmont branch of the French Bourbon family. It is a rare, if not the only, case of professional scientists with royal connections. A family which for many centuries produced military leaders, courtiers, or diplomats

now had two physicist brothers, and the younger of them was a theorist inspired with a revolutionary concept. It might seem that the quiet prince who then led a solitary life made it his task to demonstrate that a royal family could also produce offspring who deserved to be mentioned alongside the children of fishermen (Lomonosov), plumbers (Gauss), or colonial farmers (Rutherford).

And it would not be any exaggeration to say that this distant cousin of numerous kings had really done 'royal' services to science and our knowledge of the nature—for the first time in nine centuries the Bourbons produced a real king!

Apart from idle gossip, the thesis written by de Broglie excited some serious debates. The Russian physicist Ioffe heard an echo of them in the autumn of 1923 at the fourth Solvay Congress. The jovial Frenchman Paul Langevin, who had been a student of J. J. Thomson together with Rutherford, among other scientific news told Ioffe about the study of a student of his in Paris: 'His ideas, of course, are nonsensical but he develops them with such elegance and brilliance that I have accepted his thesis.'

So he admitted accepting a nonsensical thesis only because of the elegance with which it was presented. How good-natured and tolerant of him! But of course Langevin, outstanding scientist that he was, felt deep in his heart that no nonsense could be presented with elegance and brilliance; the elegance of the theory warranted its deep-seated significance.

One again recalls Einstein's words on the theory of Bohr, 'the highest musicality of mind'. Probably, he meant that a false theory could not become harmonious. And it was precisely the inner mechanism underlying this theory that Louis de Broglie was trying to explain.

Later when he was over sixty and appeared a somewhat old-fashioned revolutionary to the new generation of physicists, de Broglie explained how in his youth he had come to an understanding of the quantum model of the atom: 'The appearance of integers in the laws governing the intra-atomic quantum motion of electrons indicated, in my opinion, the interference of these motions...' To think about interference was equivalent to thinking about a certain wave process, involved in the motion of electrons in the quantized (selected and numbered) orbits. In other words, de Broglie discerned a reflection of some continuous physical events in the quantum discreteness. What could they be?

Sitting on a beach one often absent-mindedly counts the waves—one, two, three... But one does not often think that this count describes a continuous process discretely. Another such example is the ticking of a clock which gives a discrete count of the continuous oscillations of the pendulum measuring the time. Oscillations and waves result in periodic repetition of the same states. This is reflected externally as the discrete change of such states.

De Broglie suggested that the electron was related to some wave. Perhaps, this wave could lead the electron or accompany it. A simpler though stranger suggestion was that the electron itself had some wave-like property. If it was so then its behaviour as a particle should reflect the behaviour of the wave linked to it.

Take an electron travelling along the allowed orbit. Its motion is stable—it does not acquire and does not lose anything. The stability means that after each revolution around the nucleus everything is repeated precisely as before. If we observe the electron at one point of the orbit, then after a complete revolution

the electron would at this point appear exactly the same as it had a revolution earlier. But this means that its mysterious wave should at this point have the same form as it had the revolution before—if the crest of the wave occurred there, then it would appear again at this point; or if it was the midpoint of the slope of the wave then it would again be at the midpoint of the slope.

To provide for this constancy—this stability—the orbit should accommodate an integral number of the electron waves along its length. The number must be integral! If this condition is violated even to a very small extent and a tiny shift appears—the 'phase shift' in the parlance of the physicists—the electron will return to the given point in a state different from that of the previous time. The stability will be violated; the orbit will be forbidden. The state of the atom will be non-stationary.

De Broglie traced a possible reason for the strange fact of the existence of allowed pathways for the electron within the atom. Only those orbits are allowed whose length is a multiple of the wavelength of the electron! It is only along these orbits that the electron can travel in never-ending circles.

This concept immediately explains the appearance of the web, and why the allowed orbits are discrete: those closest together differ by at least one wavelength of the electron wave so that a circular gap appears between them.

Some light was thrown on the mystery of the quantum jumps, too. The electron indeed cannot find a stable state between the allowed orbits since there are no pathways there which would be a multiple of its wavelength; it thus has to cross the instability gap in one jump which cannot be subdivided into smaller jumps... When Langevin told Ioffe that de

Broglie's ideas were presented with brilliance he meant, above all else, that they gave a simple and elegant derivation of a clear formula for the suggested electron wavelength. This he had achieved using the relativity and quantum theories.

One might well predict that this formula could not do without the Planck constant h, the quantum of action—this universal standard of smallness in the submicroscopic world. De Broglie had found that the wavelength of the electron is equal to h divided by the electron mass and the electron velocity. Indeed, what could be simpler and more elegant! One could immediately calculate that for 'normal' electrons studied in laboratories (those which are not too fast or too small), the wavelength is similar to that of the X-rays; it is equal to a few angstrom units.

Now the obvious thing to do was for the X-ray spectroscopists to conduct experiments and find out whether the electron waves were real or not.

Direct experiments were desperately needed to verify the theoretical concept of the duality of the electron, as both particle and wave. Theoretical calculations however brilliant and elegant were not sufficient for the general acceptance of such an outlandish concept, which compared electrons to quanta, that is, compared matter to radiation.

The particle electron...

The wave electron...

The first concept did not need verification—the electron had originally been discovered as a particle, more than a quarter of the century back. The second concept required a 'rediscovery' of the electron more than a quarter of a century after this, which would demonstrate its new wave aspect (one which had as yet never been experimentally observed).

7

The electron was thus 'rediscovered' three years later in 1927, the crowning year of the quantum revolution. We still have to reach this peak; but for the moment we must anticipate events and briefly visit the summit just to continue with the tale of the 'matter waves'.

That was the name physicists had given to the de Broglie waves and the term, 'matter waves', fascinated contemporaries. The picture of nature again revealed something unimaginable—some 'undulation' of matter.

It now seems strange that his elder brother could not demonstrate the waves experimentally in his well-equipped private laboratory in rue Byron and this was all the more strange as experiments with X-rays were (as the French say) 'spécialité de la maison'. The fortunate similarity between the wavelengths of the electron and the X-rays could be directly seen from the elegantly simple formula of the younger de Broglie. The question seems even more puzzling if one reads what Louis de Broglie told the historians forty years later, in January 1963: 'My brother Maurice regarded the X-rays as a combination of wave and particle.' The laboratory in rue Byron thus had all the ingredients necessary for the crucial experiment—the instruments, the guiding idea, and a conducive atmosphere. But nothing was done!

Incidentally, it was precisely in this laboratory that in 1911 (before the Great War) the nineteen-year-old Bachelor of Arts Louis de Broglie was fascinated by the physical research conducted by his elder brother, and first became acquainted with the controversy surrounding the quantum ideas. Maurice de Broglie

was a secretary of the first Solvay Congress and he brought back from Brussels the materials of the discussions held there. The young Louis read all of them and found them so seductive that he converted to theoretical physics for good. But meanwhile he was involved in the experiments conducted in his brother's laboratory, and worked together with the highly skilled experimenter Alexandre Dauvillier ... Yet another reason to be amazed.

Maybe the workers in rue Byron just did not know how to conduct the needed experiments, or what to look for? But no, that they knew! One of those who formally examined the 'nonsensical thesis', Jean Perrin, asked whether the concepts presented in the thesis could be proved experimentally. Louis de Broglie gave a very clear answer: electron waves passing through a crystal should give rise to the same diffraction picture created by the atomic 'joints' of the crystal lattice as that produced by the X-rays ...So what in the end prevented a discovery of the electron as a wave in 1924—three years earlier than it happened in reality?

The reason was human nature: a bird in the hand was better than two in the bush... Louis de Broglie (now over seventy) told the historians that he had suggested the experiment to the practised Alexandre Dauvillier in 1924—but the latter refused! He was preoccupied with the experiments on television which seemed to open great prospects...

In short, things went the usual way: the abstract 'matter waves' dreamed up by the slightly unworldly prince had to give way to a common sense task of practical interest. But common sense never makes good sense when fundamental knowledge falls a victim to it. Dauvillier would never have refused to con-

duct the simple experiments suggested by de Broglie had he known that the outcome could pave the way for electron microscopy, quantum electronics and in general all the quantum miracles of modern technology. And, of course, he would eagerly have snatched at the chance had he known that his success would have been rewarded the American Clinton Joseph Davisson and the Englishman George Paget Thomson for their 'discovery of the diffraction of electrons in crystals' which was precisely what Louis de Broglie had confidently proposed to discover. Not for the first time in history it was shown that the calculating hard-nosed men in fact did not see an inch beyond their own noses.

Davisson and Thomson discovered the wave-like behaviour of electrons in 1927 independently of one another. It was found out later that Davisson had observed the electron diffraction six years earlier: but he had not understood the significance of the strange patterns he had obtained in his experiments with electrons and nickel crystals, he just could not imagine that what he saw was a wave pattern. Once again one recalls the amazingly acute words of Einstein, which many regard as a heresy: 'Only theory decides what it is that we manage to observe!'

It was a loss to physics that Davisson had managed to observe the electron wave in the Bell Laboratories earlier than de Broglie had managed to give a theoretical description of such a possibility—having appeared as a wave the electron remained unrecognized as such for six long years!

But George Thomson, a son of the old J. J., specially conducted sensitive experiments in a laboratory of Aberdeen University knowing beforehand what he would see; he thus managed to make photos showing the wave behaviour of the electron. So the father

took all the credit for the discovery of the electron as a particle, and the son was given a half-share of the credit for discovery of the electron as a wave.

An interesting discussion took place in the mid 1950s as to whether it had been a pure accident that the corpuscular nature of the electron had been discovered earlier than its wave nature. Now would the physics of the submicroscopic world have developed if the electron had instead been first discovered as a wave, rather than as a particle? These questions offer a wide scope for speculation. If one is permitted to make jokes about such a serious matter, one can say that to change the order of these discoveries the father and the son in the Thomson family would have to switch their places—and that is quite against the laws of nature.

8

Even stubborn sceptics had to accept the de Broglie theory after the successful experiments of 1927. In 1929 he was awarded the Nobel Prize, untypically soon for the Swedish Academy (Davisson and Thomson had to wait for their prizes for ten years).

The French theorist was introduced to the audience at the ceremony of the Nobel Prize presentation by the Swedish physicist Carl Oseen (an old friend of Niels Bohr and a supporter of the quantum ideas of even longer standing). Before asking Louis de Broglie 'to accept the award from the hands of our king' Oseen told him:

> 'A well-known Swedish poem has as its opening words 'My life is a wave'. The poet could also have expressed his thoughts by

saying. 'I am a wave'. Had he done so his words would have contained a premonition of man's present deepest understanding of the nature of matter.'

Those present might think that Professor Oseen had let his imagination run away with him—*could* the wave nature of the electron imply the wave nature of everything in the material world?

But it did! That was precisely what the simple and elegant formula of de Broglie implied: the wave property was an inescapable feature of any moving mass, irrespective of whether it was the mass of the electron or the whole atom; of a pellet, or the entire globe... Thus in the fable telling how the Newton law of gravity was discovered it was quite immaterial what was falling to the ground—an apple or anything else; the only aspect which mattered was that it had a mass. Actually, Newton analyzed not the fall of an apple but the motion of the Moon which was similar to a fall... Carl Oseen did not stretch the rights of the romantic poet—he could say of himself, 'I am a wave' without contradicting physics. The metaphor was that of science rather than of poetry.

Then the question naturally arises whether this means that classical mechanics from the very beginning, and always, has dealt not just with physical bodies but with our centaurs—'the wave bodies'? The answer is, naturally, yes!

So classical mechanics was unforgivably blind? Yes, it was blind: but it could not notice the wave nature of matter for the same good reason that it was unable to detect the increase in the mass of bodies accompanying an increase in their velocity—the reason being the unobservable smallness of this effect.

The formula of de Broglie gave the unprecedented new knowledge and at the same time absolutely justified all the experimenters of the past.

Could the astronomers now detect the de Broglie 'undulation' of the globe? To answer this question let us do some calculations... As you remember the wavelength of the de Broglie wave is obtained by dividing Planck's constant *h* by the mass and the velocity of the body. The larger the mass the shorter the wavelength. Assume that the globe and the electron have identical velocities; then the wavelength of the globe would be shorter than that of the electron by the same factor by which the mass of the globe is greater than that of the electron. Here are the figures: the globe weighs approximately $6 \cdot 10^{+27}$ grammes, the electron, approximately 10^{-27} gramme.

The mass of the globe is thus $6 \cdot 10^{54}$ times that of the electron.

Now, the electron waves measured in angstrom units have a wavelength comparable with the atomic size. So, to obtain the wavelength of the de Broglie 'globe wave' we divide the atomic size by a figure with 54 zeros. There is no imaginable physical event in which such a fantastic smallness could be traced!

No less illusory was the wave character of that Swedish poet who had the theoretical right to exclaim (metaphorically) 'I am a wave!'. Imagine a sturdy Scandinavian weighing about 200 pounds—10^5 grammes; his mass is greater than that of the electron by a factor of 10^{32}. We should then divide the atomic size by this factor to obtain his de Broglie wave. Can one imagine an experiment in which one could measure a length amounting to such a tiny proportion of the atomic size!? Therefore, the metaphor offered by Carl Oseen did belong, after all, to the realm of poetry rather than of physics.

Everything started from the planetary model of the atom—from the comparison of the atom to the solar system. Now a reverse comparison was in order—an attempt could be made to find the features of the quantum model of the atom in the solar system.

To do that we should assume that the planets revolve in allowed orbits. And only those orbits are allowed whose length is equal to an integral number of the 'planetary waves' of de Broglie. For the Earth this means that the two nearest allowed orbits differ by one 'Earth wave'. This is the difference between their lengths, and the gap between them is even smaller. Neither an atom, nor an electron, nor a multi-millionth fraction of an electron could fit into this gap. Such a gap is no more real than the total absence of the gap. In short, the ellipses of the allowed Earth orbits just adjoin each other continuously and practically fill the entire space. There is no discreteness in the allowed planetary pathways, and the levels of the energy of mutual attraction between the Sun and the planets do not form any ladder. The concept of quantum jumps from level to level is here quite meaningless (even supposing the planets could jump and emit quanta).

So where does this lead us? A quantum treatment of the solar system similar to that of the atom does not yield any new results compared with what has long been known from classical mechanics. This is why classical mechanics could not predict the new understanding that the matter waves introduced into science.

But in the submicroscopic world where the masses of physical bodies are all but imperceptible the wave nature becomes quite perceptible indeed. It was not accidental that it was first found for the electron—it is the lightest body in the atomic realm.

However, the heavier particles comprising the nuclei—protons and neutrons—also have a distinctly dual nature. Their wave behaviour is no less noticeable than their corpuscular behaviour. Their mass is just 2000 times greater than that of the electron and, of course, their de Broglie waves are correspondingly shorter than the electron waves—they amount to a few thousandths of an angstrom unit, that is something about 10^{-11} centimetre. Yet though it is a very small quantity, it is approximately a hundred times that of the radius of the electron—10^{-13} centimetre. Therefore this is something quite sizable in the submicroscopic world; the 'proton waves' and the 'neutron waves' are clearly significant for a correct description of the events happening in this world.

Of course, the wave nature of the nuclear particles was also proved in the direct experiments. Like electrons, they were examined for diffraction and interference. Dempster was probably the first to study the diffraction of the proton rays in crystals. Having started with photography using light rays, science can now produce pictures of objects with X-rays, electron rays, proton rays and—most important for practical applications—with neutron rays.

All the numerous physical bodies inhabiting the submicroscopic world have this dual nature. Somebody even invented a comic name for these bodies—'wavicles'—that combines wave and particles. (No less expressive terms could be found in other languages, I am sure.)

It goes without saying that all these particles—whether charged or neutral, stable or unstable, found in force fields or in matter, discovered in the atomic depths or in cosmic rays, invented by the experimenters; all the different names in different nomenclatures given to them at various times (the nucleons,

mesons, hyperons, leptons, baryons, hadrons, resonances, fermions, bosons, photons, gravitons, quarks, gluons), simply the elementary particles or antiparticles, strange or charmed—all had to be described and still are described in such terms and concepts as hitherto never had cause to appear throughout the long history of science...

Before all else, physicists had to develop a mechanics of the particle waves which would reflect this inherent duality of 'primary matter'. That was what Niels Bohr realized when in the summer of 1925 he predicted the coming 'resolute restructuring of the concepts on which the description of nature had been based'.

But little did he know that the restructuring he predicted was that summer already in full swing.

Chapter Five

Ideas and Passions

1

Now that we come to describing the culmination of the 'Sturm und Drang' period the adjectives 'amazing', 'strange' or 'queer' will appear with increasing frequency. But there is an apparent immodesty in using them. We are claiming the right to compare the ways of nature to human standards of the usual and unusual, as if man were indeed the measure of all things.

Why do we believe in this ancient wisdom? Of

course, it can be interpreted in different ways. But frankly, it smells of imposture—mankind declared itself the highest authority on nature for the only reason that there was nobody to question this claim. Apparently, it is true that no natural being but man can create conceivable and testable models of nature. But human experience is limited. Do not we stress this limitation when we call quite natural things 'amazing' or 'queer'?

Man started to find out about the Universe from 'the wrong end'. To be more exact, not from an end but from the middle—from objects and events on the terrestrial scale. Only later could man see on a larger scale using theories and instruments to probe far out into the galaxies, and, on a smaller scale, deep inside the atom.

But had man started understanding nature in a logical order—from the elementary to the increasingly complex—he would not then have experienced any agonizing drama of ideas at any stage. Everything would be revealed in the order established by nature itself; everything would happen as it does when one studies a foreign language by starting from the alphabet—and here from the laws governing the behaviour of the most elementary, most fundamental entities of matter in space-time. The infinite volume of ordered knowledge would be accumulated page by page without any omissions or references to other sources of understanding (such as an omniscient Providence). Moreover (which is the most appealing) we could enrich our concepts of nature without making sacrifices—we would not have to renounce old opinions and argue about their applicability.... And no one would be distressed trying in vain to comprehend the simplest concepts.

To prolong this day-dreaming: quantum mechan-

ics would be something like the arithmetics of physics and the relativity theory like the multiplication table; both theories would be studied by schoolchildren (of course, some without any great interest, but all without special efforts).

However, the physicists attacked the submicroscopic world not from inside but from the outside. Can one then call it strange that the primitive inscriptions found on Easter Island have proved to be more difficult to understand than the second part of Faust or modern abstract paintings?

Physicists have only one justification (or consolation): nature itself is responsible for the fact that this intelligent being had to start its study from the middle. It was nature that predetermined that such creatures should have a macroscopic scale. They cannot be the natives of submicroscopic world, they cannot be formed at the microscopic scale. There are many reasons for that. One of them is given by cybernetics—the science of information and control theories.

It is possible to build a machine which would produce its own replicas. But it has been proven that such a self-propagating machine must be highly complicated. Of course, the original is easier to produce but the process of reproduction of its replica is very complicated. Simple entities cannot do it. A virus which is capable of producing a virus is necessarily a very involved structure of a multitude of atoms; from the physical point of view this apparently smallest specimen of living matter is nevertheless a macroscopic body. But it still lacks a capacity for thinking.

Such a perfect achievement of the engineering genius of living matter as the human being could not be attained without going far beyond the boundaries

of the submicroscopic world. It is just because it is so perfect, that an intelligent creature cannot help being astonished with the elementary particles which in contrast seem to be queer entities—comic 'wavicles'. It is precisely the biological perfection of man which does not allow him to regard himself as 'the measure of all things'. Of course, not all!

And it is this biological perfection which allows any one of us to live anew the 'drama of ideas' which was experienced for the first time in the middle of the 1920s, by the creators of the mechanics of the submicroscopic world—the mechanics of the particle-waves...

2

'The quantum theory is very much like some victories', joked one of them: 'for a month or two you are laughing and then you cry for long years.'

Another once exclaimed—'If these damned quantum jumps really remain in physics I will not forgive myself for getting involved in the quantum theory!'

'...We were slipping into complete exhaustion,' a third recalled, 'and our nerves were stretched to the limit...'

The fourth one—but it was Einstein, and using a number when talking about him in this context seems incongruous—wrote to Louis de Broglie in his old age:

> 'I must have seemed an ostrich who hides his head in the sand of relativity so as not to face the wicked quanta.'

The joke about a short-lived merriment and long tears was made by Bohr's assistant, a well-known

theorist Hendrick Kramers; the famous Erwin Schrödinger was the one who wished damnation on the quantum jumps; the no less famous Werner Heisenberg remarked on their complete exhaustion, and he added, 'the incomprehensibility of the quantum theory' brought complete despair.

We could find much more evidence of this kind to give us psychological consolation. It would not be an exaggeration to say that each of the veterans of the quantum revolution, irrespective of his specific contribution to the general success, expressed similar feelings at least once. Evidence could be found in research papers, in memoirs, in public speeches, or in private correspondence. Now we understand that things could not have happened otherwise!

The general frame of mind of the physicists by the middle of the twenties can very well be expressed by the words of Sergei Vavilov. He then began with something that sounds very similar to a later and independent speech of Einstein:

> 'A contemporary physicist sometimes feels that the ground is slipping away from under his feet and he has lost any support. This giddy sensation is perhaps similar to that of old astronomers at the time of Copernicus who tried to understand that the sky and the Sun which apparently moved were stationary.' Vavilov continues rather unexpectedly: 'This unpleasant feeling is illusory, the ground under the physicist's feet is firm because this ground is'...

I wonder how a contemporary historian of physics would end this sentence? Vavilov ends it lucidly—'facts'. The ground is firm because 'this ground is facts'. One would not question such convictions.

William Bragg said something similar, but a decade earlier. He was sure that the X-rays were a flow of particles and together with his son he was conducting at the time a remarkable study of wave behaviour—the diffraction of these rays by crystals. What would be the theory he adhered to: the corpuscular theory, or the wave theory? He did not feel very happy about that. The theoretical ground had slipped under his feet, too. 'However', he wrote to Rutherford at the time, 'this can be debated separately; now I will just give you the facts.' His attitude was the same as that of Vavilov—reliable facts provided an asylum from the theoretical uncertainty.

But one cannot help thinking that it is precisely those reliable facts which provide the scientists's intellectual disquiet. It is because there appear irrefutable facts inexplainable in terms of the old theories that one has the feeling that the ground is slipping under one's feet. It happened with the Michelson experiment, with the theory of thermal radiation, with the scattering of alpha particles in large angles, and with the linear spectra of atoms...

These experimental facts, each in its own way, left gaping cracks in the foundations of the old edifice of physics. The most profound theoretical insight was needed to restore an integral picture of nature in physics that had been broken down by these experimental data.

Einstein had to discover the relativity of space-time. Planck had to discover the quantum of action. Rutherford had to suggest the planetary model of the atom. Bohr had to put forward the quantum postulates. De Broglie had to reveal the wave nature of the matter.

So what can we conclude? That it is not the facts but the reliable theories that cement together the

foundations of the physical understanding of nature, and they tend to break down under the attack of new observed data! Thus, the words about firm ground under the physicist's feet should be understood in relation to the theories. Actually, it is quite obvious: the meaning and significance of facts are revealed only after they have been explained. Only after that can we make use of them.

Once again one recalls Einstein's words that theory alone decides what is the content of our observations. Anyone can come to this opinion independently, for instance, in trying to analyze the real meaning of such a reliable observation that the Sun 'rises and sets'. Whether the Sun in fact does that depends not on our observations but on our understanding of the Universe. In other words, the theory! This declares that 'sunsets' and 'sunrises' are signs of the rotation of the Earth around its axis, and that the Sun does not have to travel around our planet at an enormous speed to produce this effect.

Looking back from our time into those distant twenties one feels tempted to alter the conclusion of Vavilov's phrase: '... The ground is solid under the physicist's feet because this ground is reliable theories.' But maybe it became clear only in our time... Had one the right to make such a change at that time?

Of course, Vavilov was psychologically quite in the right—the brilliant theories which were hardly born at the time were not regarded as absolutely reliable ones. On the contrary, it was these theories that by undermining the age-old truths re-established firm ground under the feet of the physicists and in doing so produced dizziness in their heads. They still left room for the illusion that facts by themselves could build up the foundations of knowledge. Perhaps, the

feeling which physicists had at the time was similar not only to the emotions of the old astronomers at the time of Copernicus but also to the sensation experienced by sailors of any age who reach firm land after long sailing in stormy seas—the land sways under their feet as if it were slipping away.

It was not just one but even two mechanics of the submicroscopic world that sprang to life simultaneously in the middle of the 1920s.

3

To get rid of the temptation to be continuously wonderstruck by the events of our good story I would like to yield to it just one more time.

> (Celia) 'O wonderful, wonderful, and most wonderful wonderful! and yet again wonderful! and after that, out of all whooping!'

This wonderful retort from Shakespeare's comedy *As You Like It* does not involve any mockery: Celia just had to delay the moment of discovery and to excite the searching curiosity of Rosalind. And this she managed to do—

> (Rosalind) 'One inch of delay more is a South-sea of discovery; I prithee, tell me who is it quickly, and speak apace. I would thou couldst stammer, that thou mightst pour this concealed man out of thy mouth, as wine comes out of a narrow-mouth'd bottle; either too much at once or none at all. I prithee, take the cork out of thy mouth, that I may drink thy tidings.'

Incidentally, those who tend to think that there is nothing new under the sun can eagerly snatch at Rosalind's words as a hint on the quantum principle of radiation—'either too much at once or none at all'! The emitting atoms behave as stammerers, according to Shakespeare, or as bottles with narrow necks. Thus, Shakespeare, albeit unwittingly, suggested the idea of quanta exactly three hundred years before Planck since his comedy had appeared in 1600 while Planck (as we remember) reported his inspiration in 1900...

Yes, it was 'wonderful wonderful' that the forty-year-old Niels Bohr, the recognized head of theoretical quantum physics, in announcing the coming 'resolute restructuring' of the old concepts in 1925 did not know that this was already underway. And, moreover, it was moving in two opposite directions and in two different corners of Central Europe. Physicists who hadn't even a change just to see each other at some congress or seminar had started, independently of one another.

That Bohr did not know about that at the time was due to the inherent property of any intellectual revolution: at the beginning it passes unremarked and does not recognize itself as a revolution. At first it needs this anonymity.

Moreover, both these 'revolutionaries' did not at that moment seek Bohr's guidance or approval, and that is another reason why he did not know about their work. The explanation was simple: Bohr predicted the coming revolution in conventional concepts even as he started to believe in the reality of the particle-waves, while those two—Erwin Schrödinger and Werner Heisenberg—did not believe at all in the corpuscular-wave duality of the electrons, protons or quanta.

So would it be logical to say that no recognition of such duality was necessary for the successful development of the mechanics of the submicroscopic world? At any rate, they both believed that and this was still another amazing feature of these events. Both creators of the mechanics of the submicroscopic world worked from completely different images when they thought about the submicroscopic bodies whose behaviour they were attempting to describe by their theories.

One thought that only the particles had a physical reality and their wave behaviour was a mathematical illusion. (The illusion was useful but still it was an illusion.) The other said that physical reality could be attributed only to the waves, and their corpuscular nature was just a mathematical trick. (A convenient trick but still a trick.) Therefore, they started their 'revolutions' from opposite ends.

Could their results coincide? The answer was obtained in 1926.

But they both started in 1925. Who was the first?

It is hardly possible to give a legally precise answer to this question . . . and then what can be regarded as the beginning of a scientific study? There is reliable evidence that Schrödinger started his work in the spring of that year, during which many memorable events happened in physics. There is a still no less reliable witness that Heisenberg also started his work in the spring. Then there are indications that Schrödinger's studies were stimulated back in February; but Heisenberg's work had been given a stimulus even earlier—in 1924 when he had taken part in long discussions on the life of the submicroscopic world in Copenhagen, in which Bohr's contributions had featured prominently. If we start investigating

when each of them had first become aware of this epoch-making problem then we could say that Schrödinger had also...

But the deeper we delve into history, the finer the features we are starting to distinguish and the more weak the outlines of the 'primary things'. Therefore we shall go no deeper. We shall start with Schrödinger just because it is the polite thing to do—he was fourteen years older than Heisenberg and already a professor, while the latter was still an assistant professor.

4

Schrödinger was a professor in Zurich where in 1900 the young Albert Einstein had graduated from the Polytechnical School. The school was now proud of the 'boy from Ulm', its former graduate, whom they had once failed at the entrance examinations. His every paper was carefully studied there, as everywhere.

At the beginning of 1925 Einstein published a paper in which he highly praised the wave concepts of de Broglie. Some time before that Einstein had urged Max Born to read the Frenchman's thesis: 'Though it may seem crazy, everything in it is substantiated.' Now he publicly announced that it was 'highly deserving'. Einstein's paper caught the attention of two prominent theorists from Zurich, Peter Debye from the Polytechnical School and Erwin Schrödinger from the university. Both had read de Broglie's thesis and admitted to each other that they couldn't understand his concepts; Debye suggested conducting a joint seminar with a report by Schrödinger.

Apparently, that was how Schrödinger started on

his quest. Soon he achieved more than an understanding of de Broglie's concepts—he had a veritably creative insight. According to some sources, it happened in the mountains.

That year in late winter doctors recommended him to leave the city and spend a few months in the Alpine village of Arosa—he had a lung disease. Frau Schrödinger later told the historians: 'We loved Arosa and in this quiet little Arosa came the first ideas about the wave mechanics. That was in Arosa.'

According to other sources, it was not the mountain air but the waves on Zurich lake that stimulated Schrödinger's imagination. People in Zurich told the historian Max Jammer that Schrödinger could well have leaped from the lake crying 'Eureka!' just as Archimedes had jumped out of his famous bath; Schrödinger was often seen bathing in the lake in the summer and autumn of 1925 and people said that it was there that the ideas of the wave mechanics were born.

Which anecdote is closer to the truth?

The first isn't mentioned by Jammer, and he thinks that the second one is questionable. Indeed, it is too elaborate to be true. Still it has the same virtue as the story of Newton's apple—it carries the image of the scientific problem. The fall of the apple illustrated the action of an unknown force; the undulation of the lake surface hinted at the wave principle in the behaviour of the matter that makes up the world.

One thing is unquestionable (as Peter Debye told the historians): as soon as Schrödinger had accepted the reality of the 'matter waves' of de Broglie, a most natural idea came to his mind—should not there be a similarity between the mechanics of the submicroscopic world and the mechanics of waves? It was precisely this idea that led Schrödinger to his

success whether it occurred to him in the quiet of the mountain village or among the noisy throng on the beach at the lake of Zurich. Incidentally, possibly both stories are right—the mountain one and the lake one; the fact is that the long route to success had to be covered in two stages.

It happened that he reached his goal two (!) times. But the first time he decided that he had lost his way... Many years later, in 1961 this was related in the obituary signed by Paul Dirac, another famous veteran of the quantum revolution. He wrote that Schrödinger had himself told him the story. To appreciate better these events we must recall other happenings of 1925. They did not take place in Zurich and Schrödinger knew nothing about them.

The significance of these events could be given in a few short words: a new quantum number was being born in the physics of the submicroscopic world.

5

If a new quantum number was being born then old ones already existed. But we have not yet mentioned them in this book. Now is the time to fill this gap. It is a really significant gap since the discovery of each quantum number was an important landmark on the long way to the quantum description of the submicroscopic world.

New terms, 'strangeness' and 'charm' appeared in the language of modern physics comparatively recently—in the late fifties and early sixties. The veterans of the quantum revolution could not even imagine the need for such quantum numbers in their youth. These numbers were not needed for understanding the structure of the atom, but for describ-

ing the properties of elementary particles which were not even known at that time. The exact science started to use glaringly 'unscientific' words, easy words free from the tradition of deriving scientific terms from Greek and Latin roots. These new quantum numbers reflect the freedom-loving spirit of the physics of our century bequeathed to it by the veterans, and how physicists are charmed by the strangeness of the innermost depths of matter...

One can easily guess that even the eldest of the quantum numbers was not older than the atomic theory of Bohr. It appeared in this theory in 1913. Soon it was given the name of the 'principal quantum number' since other (secondary) quantum numbers were needed.

The principal quantum number was introduced quite naturally: when the discrete sequence of the stable atomic states was discovered scientists had to introduce into the physical formulas an integral number to enumerate these states—the first, the second, the third, ..., the nth. They had to count the steps on the energy ladder in the atom or, in other words, they had to count the orbits in which electrons could rotate around the nucleus. This is all the same, of course. Naturally, such a number enumerating the quantum states of the atom immediately became known as the 'Bohr quantum number' before it was called the principal quantum number.

But soon it was found that the atom had many more allowed quantum states than the Bohr theory had revealed. In less than three years two new sequences of integers had to be introduced into the description of the atom, to number two more series of the allowed energy levels. (To make a parallel, the apartment blocks in a street are numbered by one sequence of numbers, the storeys in each block are

numbered by another and the flats in each storey are numbered by a third series.) Thus two additional quantum numbers defined a more precise (planetary) address for the electrons rotating in the atom, that is, the addresses of their orbits.

That was the work of a theorist from Munich, Arnold Sommerfeld, the same one who had not fulfilled the promise to explain the Balmer formula he had given to the wine merchant in the Moselle valley. He was fascinated with the quantum concepts of Bohr, calling the atomic model of the Dane a 'great feat', and he immediately started to work on the development of the model.

It was known to the physicists that if the emitting atoms were placed in an electric or magnetic field their spectra underwent amazing transformations. The lines split into two, three, four or more separate lines. The spectra acquired a 'fine structure', as this transformation was called.

According to the Bohr model, there was nothing to be amazed at in this effect: of course the electric and magnetic fields, each in its own way, could not help affecting the motion of the electrons which are charged particles. The ladder of the energy levels in the atom had to change in some way. The splitting of the spectral lines demonstrated that the main—Bohr—steps of this ladder were in turn themselves transformed into small ladders with two, three or more steps. New series of quantum jumps appeared and radiation was enriched with new quanta. In other words, the network of the allowed orbits became more dense. It is as if the rules of nature become more liberal allowing new possibilities for the electrons rotating around the nucleus.

Arnold Sommerfeld was the first to calculate these new possibilities. He assumed that since the electrons

were similar to the planets they were travelling in ellipses, rather than in circles (as in the Bohr model). In addition, since they travelled at great velocities one had better use the relativity theory to describe their motion. Thus, Sommerfeld introduced two improvements—a classical one according to Kepler and a non-classical one derived from Einstein.

According to classical physics, the electron velocity in the elliptically elongated orbit continuously varies not only in direction, as is the case in circular motion, but also in its magnitude. The electron has one velocity far from the nucleus, and another velocity nearer to it and according to the relativity theory, the electron mass varies if its velocity varies.

The result is that after completing a full revolution around the nucleus the electron does not return to the point where it started from; its position is displaced. The electron as it were stitches its ellipses around the nucleus. While the electron travels along its orbit this elliptical orbit itself rotates rolling along the plane of the orbit. Therefore the real path of the electron is a beautiful curve known as a rosette—tracing something that looks like a flower with many petals, such as a daisy.

Another way of reasoning is that the electron takes part in two independent rotations. The first, the rotation along the orbit, is quantized—not just any orbits are allowed but only a discrete sequence of them; the second, the rotation of the orbit itself, must have been quantized, too—the rosette cannot have just any petals, they probably make up a discrete sequence allowed by nature. Then, a sequence of integers—1, 2, 3, ... k, should be needed to number them.

But that is not all. The atom is a three-dimensional entity, while the electron orbits are two-dimen-

sional. As the electron travels along the ellipse, and the ellipse rolls along the plane of the orbit, this plane can itself rotate in space.

This is the third independent rotation in which the electron must take part. Clearly, it is also quantized: not all positions of the orbital plane are allowed, but only a discrete sequence of them. (One recalls the sequence of spokes in a wheel.) To number them the third series of integers is needed—1, 2, 3, . . ., *m*.

Thus, the two additional quantum numbers of Sommerfeld were added to the principal quantum number of Bohr in the quantum theory of the atom; Sommerfeld called them the 'inner' quantum numbers. They made it possible to give a correct description of the fine structure of the atomic spectra.

. . .the year 1916. The World War was raging. The borders of European states were closed to all personal contacts. But Rutherford in Manchester said that 'if we write at all it will be worthwhile seeing that physics is not left out in the cold'; and from Manchester Niels Bohr sent a letter by a devious route to the enemy country to Sommerfeld, praising his papers which they had received in a similarly roundabout way from Bavaria. Later, Sommerfeld received a letter from Einstein too:

> 'Your spectral studies gave me one of the most beautiful moments I have experienced in physics.'

However, it was still a mystery of the submicroscopic world that the periodic motions in the atom—rotations and oscillations—had the strange property of quantization. It was in that letter to Sommerfeld that Einstein exclaimed in his typically charming and half-joking fashion, 'If I only knew what nuts and bolts God uses!'.

Nature (God according to Einstein) was silent. It openly demonstrated its laws to all and sundry but never helped anyone to understand them.

6

There was one feature of the fine structure of the atomic spectra that still could not be interpreted. It was known as the anomalous Zeeman effect. Even the new quantum numbers of Sommerfeld were not enough to explain it.

This effect consisted, for instance, in the splitting of the yellow spectral lines of sodium into four or six close lines in a magnetic field. Clearly, the atom had some other—still unknown—quantum possibilities; the three quantum numbers describing the energy steps in the atom were still not sufficient to obtain a complete picture.

The solution eluded the most gifted theorists for a number of years. One of them—generally regarded as a genius—recalled later how he had become addicted to the problem when he worked at the Bohr Institute in 1923:

> 'A colleague who met me when I was aimlessly wandering along the beautiful streets of Copenhagen said to me: "You are looking very unhappy". I answered heatedly: "Can a man look happy if he is thinking about the anomalous Zeeman effect?" '

The ardour of reply could be attributed to the fact that the unhappy man was quite young—just twenty-three. His name was Wolfgang Pauli.

He was an extremely presumptuous young man but fortunately, he was also extremely capable. At nine-

teen he once declared after listening to a lecture delivered by Einstein in Munich: 'You know, everything that Mr. Einstein told us isn't so stupid after all...' But his own large paper on the relativity theory had an even greater success than this unforgettable quip. Einstein said that he understood his own theory better after reading the paper by the young Pauli.

That very spring of 1925 when Erwin Schrödinger and Werner Heisenberg were nurturing the concepts of the mechanics of the submicroscopic world, Wolfgang Pauli wrote in despair to another, even younger, theorist:

> 'Physics is too difficult for me and I am sorry that I haven't become a comic film actor, or somebody of the kind, just so that I would never hear anything more about physics.'

It is a quite unexpected admission psychologically since it was precisely that spring when he published the historic paper that opened the way to a solution of the Zeeman mystery, which had tormented him. Pauli found, finally, a new quantum number ten years after Sommerfeld had introduced his numbers.

Pauli discovered still another feature of quantum discreteness in the submicroscopic world. He guessed that this feature was a property of each electron in the atom, rather than of the atom as a whole; he called it the 'two-valuedness' of the electron. It was an abstract notion; he did not build any model and did not attempt to visualize such a feature.

The only question that mattered for him was that the quantum possibilities of the submicroscopic world were at least doubled... The ladder of the allowed energy levels in the atom became even more complicated... The anomalous splitting of the spectral

lines could be interpreted correctly... In addition, many other phenomena were now open to theoretical explanation.

Was not what Pauli discovered a new type of rotation of the electron? (Physicists were used to the idea that quantization always involved some kind of rotation.) Pauli, however, was strongly opposed to any imaginable—classical—representation of any quantum phenomena. When William Bragg Sr. joked that Bohr's model suggested to physicists that they use the classical laws for three days in a week and the quantum laws for the other days he meant to say that it was logically inadmissible. Nobody knew that better than Bohr himself and his assistants. Pauli was among them. But he still did not know how to work a full quantum week.

Wasn't that why Pauli wrote to his young colleague how he envied film actors, despite his own great success of that spring? So far he demanded that the classical concepts be banned from quantum physics. Other physicists were wary of his biting tongue: he was a merciless opponent but, as we have seen, he did not spare himself, either.

The young colleague addressed in his letter was the twenty-year-old American Ralph Kronig. He had just committed what was an unforgivable sin in Pauli's book—he had suggested a semi-classical model for the 'two-valuedness' of the electron.

He had found yet another form of rotation for the electron—not around the atomic nucleus but around its own axis. The planets rotate in this way. The 'two-valuedness' suggested by Pauli had in Kronig's model the sense that it is this rotation that is quantized; if a certain position of the electron is fixed the second possible position would be the opposite one. Other positions are forbidden. The electron looks

like a strange compass whose magnetic needle can point only to south and north, or west and east, or north-west and south-east... That was the 'two-valuedness'. Having made the necessary calculations Kronig immediately obtained a correct formula for the splitting of the spectral lines.

However, Pauli said that all that was just a 'clever trick', nothing else. He did not agree with the Kronig model. Then Bohr himself rejected it, too. Others followed in their footsteps. The ensuing dramatic situation is one of the better known in our good story.

Ralph Kronig was too young and too inexperienced; he gave up. He did not dare to send his paper to be published. One of the irrefutable objections against his model is clearly understandable.

If the axis of rotation of the electron had indeed looked like the magnetic needle of the compass then the electron itself would have resembled a rapidly spinning top. That was not a problem in itself, but the speed of this top had to be enormously high. It had to be so high that the parts at the periphery of the electron—the top with the radius of the order of 10^{-13} cm—would have a speed higher than the speed of light. And that was not allowed by the relativity theory.

Nevertheless, two equally young Dutch theorists, Goudsmit and Uhlenbeck, came to the model of the rotating electron independently of Kronig in the same year, 1925. They worked in Leiden.

The chair at Leiden University from which Lorentz had retired was now occupied by a prominent theorist and an extremely kind man, Paul Ehrenfest. Before the First World War he had worked for five years in Russia. He was friendly with many Russian physicists and he was a close friend of both Einstein and Bohr. There was no other in the history of the quantum

revolution, even perhaps including Wolfgang Pauli and Lev Landau, who was better described as a 'creative critic'. His criticism was always not only very keen but invariably helpful and kind.

Of course, the young scientists, excited by their idea, immediately approached Ehrenfest. They were not stopped by the difficulties it presented though later Uhlenbeck recalled:

> 'Our enthusiasm was damped to a considerable extent when we found that the speed of rotation at the surface of the electron should be many times that of the speed of light!'

Ehrenfest commented that it was 'either very important or nonsense'. He told the young men to write a short communication to a scientific journal. They did that. Sympathy is always encouraging. After having given this communication to the professor, however, they gave way to incapacitating doubts, as did Kronig. Thirty years later in 1955, when he inherited the chair of Lorentz and Ehrenfest, Uhlenbeck recalled:

> 'We with Goudsmit felt that maybe it was better to refrain from publication but when we talked with Ehrenfest about that he answered: "I have sent your communication to the journal long ago. You both are young enough to allow yourself a folly or two!"'

Thus, the concept of 'spin' in the physics of the submicroscopic world is forever linked to their names. A fourth quantum number was introduced to physics instead of the vague 'two-valuedness' of the electron.

Only later regrets had been left to Kronig, as he recalled many years later. It was his bad luck that

he relied on the judgement of Pauli who was at the time too young to act as Ehrenfest did. It was the extremism typical of young people in general and in Pauli's nature, that prevented the latter from accepting classical images for the quantum features of nature. Pauli was immensely respectful of Bohr but twice that year he was rude to him and only because Bohr was inclined to recognize the models of Kronig, and of Goudsmit and Uhlenbeck.

Actually, Pauli was right—this effect was a purely quantum one and could not be described in classical terms. That was why the impossible velocities higher than that of light appeared in the spin model. But the arbitrary image of the electron-top served the theory well, as did the image of electron-planets revolving along the classical orbits. A year later, Pauli himself had to admit it, but only just!

Van der Waerden, who was a theorist, a historian and a rigorous mathematician, wrote later that 'there are no grounds for reproving Pauli'—i.e. for dooming Kronig to regrets. It is a dryly logical conclusion—'there are no grounds' but the development of science doesn't progress in a dryly logical way. What Ehrenfest did was more daring and more humane. And finally it was he who proved to be right in the history of physics.

On the other hand, Pauli himself deserved sympathy at the time. Three years before these events at the age of twenty-two he was invited by Niels Bohr to work in Copenhagen. Contemporaries remembered how bluntly self-assured was this fighter for the purity of theory (a little too stout for his age) in his answer to Bohr: 'I won't have trouble with physics but what I'm really afraid of is the Danish language!' Compare that with his self-abasement quoted earlier—'physics is too difficult for me'! He explain-

plained to Kronig why it was difficult: 'physics is again in a blind alley ...'

Again? But when did it happen before? This is exactly what Lorentz had said at the first Solvay Congress in 1911, before the Bohr theory was developed. Einstein experienced similar feelings just before developing the relativity theory. Planck had such misgivings even earlier—before he created the theory of quanta. To go even farther back...

'I wouldn't be surprised if I learned that similar feelings were prevalent in the period before Newtonian mechanics emerged', remarked Ralph Kronig, when telling about his misadventures in the history of the rotating electron. That was in the spring of 1925, just before the mechanics of the submicroscopic world had taken form. Events repeated themselves as if manifesting some basic psychological law.

7

Staying in the spring in the quiet mountain village of Arosa, and sitting by the lakenside in Zurich in the summer and autumn Erwin Schrödinger tried to avoid any—even scientific—distractions in order to concentrate on the search for the mechanics of the matter waves. He had even heard nothing about the birth of the new quantum number.

He was punished for his ignorance, but in a very peculiar way...

When Bohr tried to rescue the planetary model of the atom he started from the simplest atom—the hydrogen atom—on Rutherford's advice. Schrödinger did exactly the same when attempting to develop the mechanics of the de Broglie waves.

Though the atom has no boundaries or walls and

each wave, in principle, is infinite, the wave-like electron in its allowed orbit is in a trap—de Broglie demonstrated that the electron's motion along this orbit is stable because the orbit length is equal to the electron wavelength multiplied by an integral number.

The appearance of an integral number gave hope of deriving the quantization of the orbits—a discrete sequence of them—from the classically continuous motion of the electron-wave. Schrödinger started to formulate the relevant wave equation.

To derive a wave equation... this meant that its solution would be graphically represented as waves in the space of the atom. A certain quantity symbolically describing the states of the atom would vary in a wave form from point to point, and from moment to moment.

The variation of this quantity would reflect the variation of the atomic states. If the equation has been properly derived its solutions will reflect the sequence of the stable states in which the atom does not emit energy, and all the transitions between such states when energy is emitted in the form of quanta. In other words, the solutions of the wave equation will agree with the atomic spectra, or with the sets of the known quantum numbers.

Schrödinger decided to select a not too popular letter of the Greek alphabet for denoting this 'certain quantity' which was mathematically clear but physically still rather mysterious: the letter psi (ψ). Did he realise that soon this letter would be the most popular one in all the papers and books on the physics of the submicroscopic world? Perhaps he did.

He was deeply convinced that the intra-atomic mechanics had to have a wave form. He wrote at the time:

'A moving particle is nothing else but the foam on the wave radiation forming the matter of the world.'

Was it not the waves of Lake Zurich that had whispered these magic words to him? It had some physical justification, though admittedly rather a flimsy one. When the wind produced random foamy crests on the surface of the lake that was an indication that the water waves of different wavelength and different amplitude had successfully superposed each other: near the crest they were mutually cancelled out and at the site of the crest were mutually intensified. A moving 'wave packet' was created. This term was long used in physics in the study of interference of waves.

Why could we not admit that the electrons (and, in fact, all microscopic particles of matter) were the packets of the interfering matter waves? But then there were no wave-particles having a dual nature! We had to deal only with wave-waves, and everything was continuous—that was the dearest thought of Erwin Schrödinger, his fondest hope.

Meanwhile, the waves on Lake Zurich clearly demonstrated the unreliability of the concept of wave packets for the foamy crests soon faded away. Each of the waves comprising them moved in its own way and they inevitably spread out. Even the mathematical wave packets were unstable formations. They could not be used to build up the long-lived matter of the world. All that was left was foam...

One might take any superpositions of Schrödinger's waves or de Broglie's matter waves but they were not suitable for producing particles of matter. However Schrödinger kept on thinking, 'Time will tell...' A physicist told the historians that Schrödinger not only thought that but even declared it in

public. A true understanding of the physical meaning of 'psi' came later—and it was not Schrödinger's doing. This happened much later when his famous equation had already been well applied in the mechanics of the submicroscopic world.

But how could it be successfully used if the idea of the author was so bad? In fact, only one part was bad: the philosophical desire to recover the inviolate continuity of nature by describing everything in terms of waves. The possibly constructive role of the waves was overestimated; but the 'matter waves' by themselves were a true physical image of what was indeed happening in nature. The concept of the electron-particles was expelled but they were retained under the wave pseudonym of the 'foam'.

Schrödinger denounced the quantum jumps along the energy ladder in the atom. (We have heard already his damning remarks and shall again hear more of them.) Yet it was he who found a wave justification for the quantum numbers that enumerated the jump-like sequence of the larger and smaller steps in this ladder. In short, Schrödinger's equation with a mathematical impartiality gave an excellent description of all that its author rejected due to his philosophical prejudice. The equation was soon put to good use in physics.

However, it did not happen immediately. To be more exact, Schrödinger could not manage to give it a proper form at once. That was just because he was unaware of spin. Such was the irony of history: precisely at that moment when the prominent scientist hoped to eliminate all discreteness from the picture of the submicroscopic world yet another discrete feature appeared in this world—the electron spin. But he did not know about that and, therefore, could not incorporate it in his theory.

Apparently, when he descended from the mountain village of Arosa into the summer Zurich of 1925 he had the first version of his equation with him, or a method to derive it. Later he told Paul Dirac:

> '...I immediately applied my method to the motion of the electron in the hydrogen atom, properly taking into account the formulas of the relativity theory for such an electron... The calculated results did not agree with the observational data... I was deeply disappointed, decided that the method was unsuitable, and dropped it.'

He dropped it!—at this moment the fate of wave mechanics was hanging by a thread. The method was actually quite suitable and the development of physics would have been unforgivably delayed if Schrödinger had then abandoned his method altogether. His failure was caused by another factor and he just could not identify it. He assumed that he had 'properly' taken into account the relativity theory; but to do that really properly he had to take into account the inherent rotation of the electron, its spin, quite apart from the effect of its enormous velocity.

Our story cannot go into scientific details to explain why he was unable to do that. This was the work of the scientist whom Schrödinger later told about his temporary failure—Paul Dirac, who in 1928 derived the relativistic equation for quantum mechanics (which was a no less great achievement than that of Schrödinger). Schrödinger, however, could do his work only in two stages. Fortunately, the first disappointment did not crush him. His concept was too personally dear for that.

The summer and then the autumn of 1925 had passed. He still knew nothing about the new quantum

feature of the submicroscopic world, the electron spin. But once on looking through his formulae he accidentally located his error... He saw that it was too early to introduce the relativity theory—something was lacking for that. So he tried to simplify the problem, to solve it with a lower accuracy than he had wanted at the beginning. Immediately he did that the equation yielded results that agreed with the experimental data.

It was one of those cases of 'the simpler the better'... For instance, one can easily imagine how Niels Bohr might have lost his way if he had attempted to follow the Einstein mechanics from the outset. He would have found that there were no circular orbits in the planetary atom, and that the electron travelled along a complicated rosette. The obvious concept of the ladder of energy levels agreeing with the Balmer formula would be unrecognizably distorted, and he would have been deeply disappointed, while standing at the very door of a great discovery... Striving prematurely for a complete understanding of the atomic structure he would have missed an incomplete but decisively important understanding not hidden by such small details. It is not accidental that Rutherford used to repeat the Latin saying 'Festina lente' (make haste slowly).

By the end of 1925 Schrödinger thus had a wonderfully effective, though not completely accurate, wave equation. Then he started to write a series of papers.

The first was submitted to *Annalen der Physik* on January 27, 1926. Almost a year had passed since that day unmarked in history when two theorists from Zurich had admitted to each other not understanding the concepts of de Broglie, and arranged for a discussion of his thesis. Now one of them had

developed the wave mechanics which would soon gladden the hearts of all who felt themselves 'in a blind alley'.

Amazingly, the gladness was not diminished by the obscurity which still enveloped the psi-waves discovered by Schrödinger.

8

In contrast to other physicists, the second scientist who started developing the mechanics of the submicroscopic world, the young Werner Heisenberg felt no joy but was rather confused when in the spring of 1926 he first learnt about Schrödinger's success. An obvious explanation would be a jealousy born of rivalry—but no: the psychological turns in our good story are typically much subtler and more unexpected.

Heisenberg developed his version of quantum mechanics earlier than Schrödinger though he had started almost simultaneously with him. And he also published it earlier, back in 1925. His priority could not be questioned. Moreover, he could not suspect that the professor from Zurich had suggested a better theory—the differences between them were too glaring. The theorist from Göttingen was struck by a sudden suspicion: 'Both of us are completely wrong...' Both of them wrong! That was what terrified him. Then everything they had done was nonsense.

That was how he described his feeling of the time many years later in the interview with Thomas Kuhn. How did this feeling arise?

Let us return to the spring of 1925 and to Göttingen in Germany. As a lung disease exiled Schrödin-

ger to the mountains, a complaint of another kind sent Heisenberg to the seashore. Fate had also provided him with a period of solitude. The spring blooming of flowers brought on an acute attack of hay fever; his face became swollen, his eyes were bloodshot. Max Born, his superior in Göttingen, did not hesitate in giving him a leave of absence. He advised him to go to the island of Heligoland with its health-giving sea breezes, and where there were only bare rocks and no irritating pollen.

Smooth rocks on the beach, the thundering of the breakers, the foamy crests of the waves... Indeed, there was much more at the shore of the North Sea than at Lake Zurich to stimulate thinking in terms of waves. But Heisenberg was casting his nets in different waters. He gazed at the beach and saw other things—precipices in rocks, deep gaps between the sea waves...

He thought about the electron-particle. He regarded the quantum discreteness as being given by nature—they did not need any justification in terms of the continuous waves. The quantum jumps did not terrify him by their inexplicability; neither he nor his student friend in Munich, Wolfgang Pauli, were scared off by them.

They were both students of Arnold Sommerfeld who was fond of the 'quantum magic in spectra'. Perhaps it was he who instilled in them his aversion to 'partial models', and, at the same time, his Pythagorean passion for the harmony of quantum numbers (which some in Munich ironically called 'atomysticism').

Yet both outstanding young scientists—Pauli was just a year older than Heisenberg—felt that they owed incomparably more spiritually to Niels Bohr. They both had spent some time as his assistants in

the Copenhagen Institute. They both had appreciated that Sommerfeld was concerned, in his words, with the 'techniques of the quanta' while Bohr was dedicated to the search for the 'philosophy of the quanta', —and the enormously gifted young men valued that much higher.

Heisenberg came to Göttingen after spending the winter in Copenhagen. He was all set for resolutely restructuring the former concepts, as Bohr said somewhat later. Going on his trip to Heligoland he already knew what he wanted to find... His starting idea was simple: should not the mechanics of the submicroscopic world deal only with observable quantities?

Why only the observable quantities? Because this strange submicroscopic world has quantities which are, in principle, unobservable. It would be purposeless to include such quantities in a description of the events in this world—the description would get out of control. Even worse, it would be physically meaningless because it would be unknown what was described by them.

The quantum jumps are the main events in the intra-atomic mechanics. But they constitute clear violations of the continuity of electron motion. Therefore, any attempts to describe these electron jumps in traditional terms—as displacements in time and space—are obviously doomed to failure. Not that the orbital motion of electrons is easier to deal with.

Let us illustrate it.

When the classical astronomers referred to the planetary orbits they knew what they were talking about—the motions of the planets illuminated by the Sun are observable. The quantities in their formulae can be measured. On the other hand when the atomic physicists talk about the electron orbits, the argu-

ments are validated only by analogy—the orbits themselves are unobservable. We cannot illuminate the electron and trace its motion: the quanta having a comparable mass and hitting the electron will knock the electron out of its way, and there will be nothing to measure. The celestial mechanics would also lose their reliability and become useless if the fluxes of sunlight were capable of knocking the planets out of the celestial orbits they prescribed.

Heisenberg thought that the history of physics in the 20th century was supporting his ideas. Did not Einstein refuse to recognize absolute time—a common time for all moving bodies—just because no, even imaginary, experiment could prove its existence?! There is no Time but many kinds of times, all of which are relative and related to the motion of bodies. The true mechanics could deal only with these 'times'.

Rutherford's concept of the electron-planets possibly and even probably is no more than an illusion. What is observable? Only that the atom varies its energy discretely. This discreteness testifies to the existence of a ladder of allowed energy levels in the atom. The indivisible jumps along this ladder are indicated by the emission of light in entire portions—the quanta. This is genuine knowledge.

What can be measured here?—the frequencies and the amplitudes of the oscillatory processes which take place in some way in the atoms and give rise to the radiation quanta. The spectral lines describe the frequencies and the amplitudes by their colour and their intensity. The frequencies describe the energy of the quanta—the higher the frequency the higher the energy of the quantum. The amplitudes describe the probability of the emission of the quanta—the higher the amplitude the higher the probability of the emis-

sion. (Therefore the line is more intense.) This is genuine knowledge.

Sets of such observable quantities yield reliable information on the significant events in the life of the atom, on the quantum transitions between its stable states... Should one not start developing the mechanics of the submicroscopic world from this point? That was Heisenberg's concept, or, more exactly, a brief outline of it.

He started working on his theory of the atom even before the flowers came into bloom. Of course, like Bohr and Schrödinger, he started with the simplest atom—the hydrogen atom. But he did not get any results in Göttingen. At first he lost his way.

Apparently, it happened at precisely the same time when Schrödinger lost his way in the mountain village of Arosa. (Historians might find out if such a curious coincidence really did happen.) But the causes of their failures did not look similar even from the outside. Schrödinger did not know about a new physical fact; but Heisenberg was not aware of the existence of an old mathematical method for calculating such quantities as the discrete sets of observable variables.

Physics was not concerned with such things. No physical experience could be of any use to the young theorist. All that was left was to look for a suitable format for writing down these sets in terms of mathematical symbols. And a way had to be found to deal with them. Heisenberg set to working on that. Later, Max Born was reported as having said, admiringly, that one had to be a really intelligent ignoramus to not know the proper mathematics but be able oneself to develop the appropriate mathematical method if one needed it! Heisenberg struck terra firma even before his flight to Heligoland.

In principle, quantum transitions can occur between any two energy levels in the atom. Thus, a unified notation had to cover all the possible quantum jumps along the energy ladder. That seemed like trying to find a format for writing down all the results of a tournament in which everyone plays with everyone else. The participants in the tournament here are the stationary states. They are enumerated by the quantum numbers. The results of the games between them are the emission or absorption of the quanta. A square tournament table is suitable for writing down at once the results of all the possible games, one table for the frequencies and another for the amplitudes.

Heisenberg had a day on Heligoland—sea, solitude, quiet,—when he saw light ahead. Evening had fallen. Forty years later Heisenberg told the historians:

> 'I was extremely excited and it was just early in the morning. I got in a state of great excitement because I saw that it worked out so nicely... I worked all night and I made many slips in the calculations. I decided I would go out for a walk and so I did. I rather half-climbed on one of the cliffs of Heligoland just for excitement and I felt "well, now something has happened". Then I started writing on a paper.'

There was another moment when he felt that everything was wrong. He saw that the algebra of the square tables did not always satisfy the age-old law, A multiplied by B is equal to B multiplied by A. In nature this *commutativity of multiplication* was always thought to be self-evident; but when Heisenberg multiplied different observable quantities he found that he could not transpose them without changing the result:

—'I was terribly alarmed by that', said Heisenberg later.

At that moment the future of the mechanics of the submicroscopic world was hanging by a thread just as in the same spring in Arosa. In contrast to Schrödinger, however, Heisenberg did not drop his method for a few months—and that was why the Heisenberg version of the quantum mechanics appeared some months earlier than the Schrödinger version.

After his initial alarm with the inequality $AB \neq BA$ the young theorist affected an inexplicable but happily care-free attitude to it: 'But then I said to myself: "Fortunately, I don't need it, fortunately it is not very important."' Little did he think that it was precisely that feature that would prove crucial, and that would reveal what is, perhaps, the most unexpected of all the nonclassical laws of nature!

His care-free complacency proved to be extremely useful. Despite alarm signals from his common sense, he continued his work after returning from Heligoland. There were many more moments of confusion that June in Göttingen. Not once he had to suppress the desire to throw his papers on to the fire. Still he overcame almost everything.

Yet there was one inhibition he could not overcome—his reluctance to show what he had written to Max Born or to send it to Niels Bohr. Born, true to Göttingen traditions, always asked for mathematical rigour. Bohr, true to the Copenhagen tradition, asked for a reliable physical substantiation. (Heisenberg himself told that to the historians.) His paper lacked both. Still he wanted somebody to look at the paper and to give him friendly criticism.

In early July he sent his paper to Wolfgang Pauli in Hamburg. In his excitement he could well have

echoed the words of Ehrenfest: were they not young enough to allow themselves a folly or two? This time Pauli's judgement was generous. This is the more remarkable since he, in his turn, did not agree with all the ideas of his old friend: 'There are many more observable quantities in the atomic world than are dreamed of in your philosophy, Heisenberg.' (This is how Bohr's assistant Leond Rosenfeld expressed Pauli's views.) What was it that attracted this most severe of critics?

It was that Heisenberg dared to reject completely all the traditions of classical description for motion in the submicroscopic world. Pauli saw a crack appear in the wall of the blind alley. Immediately he stopped envying film actors. In the autumn of 1925 he wrote to Kronig in quite different words from those of his spring letter: 'The Heisenberg mechanics restored to me hope and the joy of life'.

But how can we understand why this mechanics was valid if the starting position of the Göttingen theorist was clearly wrong? He did not allow for the duality of the atomic particles. Of course, he did that in a way different to the professor from Zurich: the latter hated the corpuscular nature of the waves while he hated the wave nature of the particles.

Fortunately, that was just a philosophical weakness. In fact, Heisenberg kept close to nature. Eliminating the waves he still retained something 'oscillatory' in the atom—without that the frequencies and the amplitudes would not have any physical sense. But if there are oscillations there are waves, one cannot exist without the other; both manifest continuous periodicity. Thus a continuity feature slipped into Heisenberg's theory, in addition to the discreteness of the quantum jumps. This version of the quantum mechanics could also therefore be highly useful for the study of nature.

Having received Pauli's approval, Heisenberg finally gave his paper to Born, his superior, asking him to 'Do with it anything you think proper'.

Max Born read it the same day. It was not easy for him: he thought that it 'looked rather mystical but was undoubtedly true and profound'. He wrote that to Einstein, and he sent the paper with his recommendation that it be published. However, he was also bothered with the preposterous formula $AB \neq BA$... It faintly reminded him of something he had known long ago but he just could not identify what it was.

Later he retold this story on numerous occasions varying the details:

> 'One morning I saw the light—I remembered the algebraic theory that I had studied at the university. Such square tables were well known to the mathematicians; they were known as *matrices* and had a special rule of multiplication. I saw that the Heisenberg multiplication was nothing else but an element of the matrix calculus. Now we could move further on. I was as excited as a sailor who had sighted the long-awaited land after a lengthy voyage...'

(Still another moment of excitement after yet another period of despair. Yet another joy after yet another depression—and these will keep appearing till the end of our tale.)

In those July days of 1925 Heisenberg was in Cambridge, at the Rutherford laboratory where he delivered lectures. He did not know that his mechanics had by that time been christened the 'matrix mechan-

ics', he did not use this word when he made a report on his theory on July 28th to the Kapitsa club, perhaps, for the first time in public.

The young physicists belonging to this loose community centred around Pyotr Kapitsa took their seats on the floor around the fire-place as was their custom, and listened with an unquenchable interest to the first version of the quantum mechanics. The interest was literally unquenchable: the author could not yet quench the puzzlement of the listeners and answer all their questions. First of all, they were puzzled with the physical meaning of the strange noncommutativity of multiplication...

But the man who had discovered the quantum jumps, Niels Bohr, learned about the 'resolute restructuring' that had taken place later, only in September of 1925. He received a letter from Heisenberg: 'I have written a paper on quantum mechanics which I would like to have your opinion about.' It would be better for him if he had dared to do that earlier! The fact was that Niels Bohr was then practically the only theorist who was not puzzled on seeing for the first time the formula of multiplication for matrices... Perhaps, he understood the formula $AB \neq BA$ immediately. At any rate, he soon wrote:

> 'It can be hoped that a new era has opened for mutual stimulation of mathematics and mechanics. Perhaps, physicists will at first be sorry that in our understanding of the atom we cannot overcome the limitations on the normal methods for describing nature. But one would like to think that this feeling will be replaced by a gratitude to mathematics which provides us with an instrument for advancement in this field.'

Yes, the matrix method using something that looked like tournament tables was quite unusual. But it was highly promising—and it was precisely at that time that Schrödinger was close to a successful completion of his wave mechanics.

9

But what then was the cause of Heisenberg's panic ('we both are hopelessly lost!') when he learned from Pauli's letter about the Schrödinger wave mechanics, in the spring of the following year (1926)? Why 'both'?

When the famous scientist explained his feeling at the time to the historians almost forty years later he smilingly compared himself and Schrödinger to two mountain climbers searching for a way to the summit in fog.

When the fog started thinning they both saw at a distance and from two different directions the cherished summit. But how utterly different were the landscapes before their eyes near their goal! One saw sheer rocks (the quantum jumps) and the other, smooth slopes (the matter waves). Could they both be sure that what they saw was the same mountain? No, they had a strong suspicion that, perhaps, both had lost their way...

Later the confusion was cleared. Of course, it first gave way to Heisenberg's conviction that only he was right and that it was Schrödinger who had lost his way. Naturally, the professor from Zurich thought almost the same about the quantum mechanics suggested by the Göttingen theorist. Neither were too fastidious in their choice of expressions.

Heisenberg wrote to a friend that 'The more I think

about the physical side of the Schrödinger theory the more revolting it seems to me.' Schrödinger did not leave that comment unanswered: 'This difficult (Heisenberg) method seemed to me depressing if not revolting... It was devoid of any clarity.' At the same time, Schrödinger did what Heisenberg did not—he immediately attempted to find out whether both methods described the same thing in terms of different languages. Very soon he demonstrated by means of rigorous mathematics that it was exactly so!

The wave mechanics and the matrix mechanics were not at all at odds. They ran parallel courses as if they could be literally translated: it was as if the first was saying to the submicroscopic world 'I love you' and the second was saying 'Je t'aime'... In conclusion, I would like to tell of an episode that happened at the time and which gives a very apt psychological illustration.

In the summer of 1925, when the wave mechanics was not yet in existence and the matrix mechanics had just appeared two theorists from Göttingen went begging to the great David Hilbert, the established head of the Göttingen mathematical school. They asked the world-famous scientist to help them with the matrices. Hilbert listened to them and said something quite remarkable—each time he had to deal with these square tables they appeared in his calculations as a sort of 'a byproduct' in the solutions of the wave equations, 'So, if you look for the wave equation which has these matrices you can probably do more with that.'

According to the American Edward Condon, the theorists were Max Born and Werner Heisenberg. The episode ended in this way:

'They had thought it was a goofy idea and that Hilbert did not know what he was talking about. So he was having a lot of fun pointing out to them later that they could have discovered Schrödinger's mechanics six months earlier if they had paid a little more attention to his words.'

One can hardly find a better example demonstrating the blindness of a one-sided approach. At the same time, it shows how true was the classically 'impossible' concept of the wave-particles. The two mountain climbers had seen the same mountain: if it were otherwise, only one of the two versions of the mechanics would be true—either the matrix mechanics or the wave mechanics. How they were naturally amalgamated or, better to say, fused into what is today known as the *quantum mechanics* of the submicroscopic world.

One could have expected that the struggle between those initial ideas would be over. But no, the passions still ran high. More than a quarter of a century later, in the fifties, Louis de Broglie once said in recalling the turbulent events of our good story that 'The most dramatic event of modern microphysics was the discovery of the duality of the wave-particle, as is well known.'

'As is well known'! The veterans of the quantum revolution knew that from their own experience, which had a duality of its own—happy and saddening at the same time.

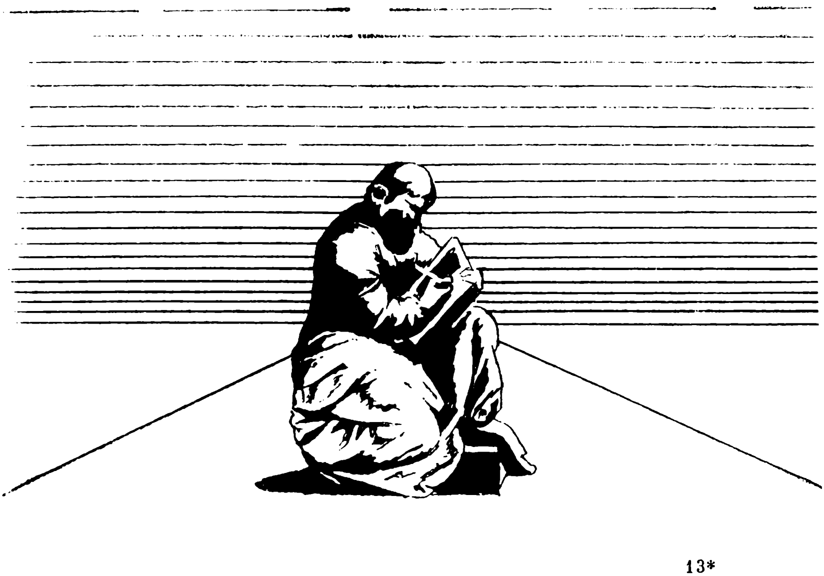
Chapter Six

The Route to the Summit

1

Thus, two versions of the mechanics of the submicroscopic world had been developed.

Both proved useful, both yielded results which agreed with the experimental data. This agreement was so remarkable that the famous Enrico Fermi, the future creator of the first nuclear reactor (who was equally great as a theorist and as an experimenter) declared to his students: 'There is no need for such a good agreement!'



The Italian Fermi who was of the same age as Heisenberg made an invaluable contribution to the development of the quantum concepts. His words are quoted in the memoirs of a prominent theorist of the next generation, Austrian-born Victor Weisskopf; he was of the same age as Lev Landau under whose guidance he worked for some time in the Soviet Union. In his turn, 'Long Vicky' as Weisskopf was known in Copenhagen was fascinated by the mechanics of the submicroscopic world: 'A fantastic discovery!'

Nobody could predict in 1926 that:

—in 12 years time the first source of nuclear energy would be discovered in the uranium fission reaction (Germany, 1938);

—in 19 years the first atom bomb would be tested at the end of the Second World War (USA, 1945);

and 28 years later the first nuclear power station would be built (USSR, 1954).

The atomic age sprang into human history from the quiet of the laboratories and low voiced academic discussions. People saw its coming terrifying and magnificent, illuminated in the dual light of great tragedies and great hopes. It was as if quantum physics was doomed to duality on all levels—from the submicroscopic scale of the depths of matter to the macroscopic scale of terrestrial history. No discoveries in pure science, however fantastic, have had such rapid and loud repercussions as the appearance in the middle of the twenties of the doubly nuclear mechanics of the submicroscopic world.

Yes, at first it was doubly unclear. In both its versions physicists had to grope in the dark as soon as they had crossed the doorstep. In the wave mechanics it was the mystery of the Schrödinger psi waves; in the matrix mechanics it was the enigma of the

matrix multiplication. Everything was right mathematically but the physical meaning of both was either puzzling or controversial.

'It should be noted that the development of the mathematical apparatus of the quantum mechanics preceded a physical understanding of the atomic mechanics', Werner Heisenberg explained when he delivered lectures in the United States for the first time. One can easily imagine how confused and unbelieving the American students were—teachers always had told them that things in science happened in the reverse order, first the meaning and then the form. But in this newest science everything was amazingly different, everything was topsy-turvy!

2

Soon Schrödinger realized that his dream of building up particles from the psi waves was hopeless. Though the formulae of the wave mechanics delivered the goods his own interpretation of these formulae 'could not stand', in the words of Max Born. The fault lay with the wave packets—they spread out.

A case had been found when everything happened as Schrödinger desired, mathematically and physically: under specially simple conditions the wave packet could travel along a straight line and be stable. But that was a very special—not at all typical—case. (Incidentally, Heisenberg did his best to prove that it was exceptional; probably, he was quite satisfied if not pleased when he struck this blow against the wave illusions.) Schrödinger's illusions were based on the existence of the case when the waves created something like a particle. He hoped that development of the theory would transform the exceptional into the general and his dream would be fulfilled.

In contrast to the electromagnetic waves, the mathematical psi waves could not have any physical effect: they did not contain the energy-mass; they were not born from a force field; they could not be used to transfer information at a distance. Nobody could invent an emitting psi station or a receiving psi antenna. Only a science fiction author could dare make such an invention but then his fiction would immediately become unscientific. A natural question: if they are immaterial then why all the trouble about their physical meaning?

But are not the classical orbits of the planets or the paths of the raindrops immaterial? After a planet or a raindrop passes from a point their material moves away from it but the sequence of these immaterial points traces the line of the mechanical behaviour of a planet or a raindrop in space. The line is determined by physical laws and therefore it is packed with physical sense.

No classical lines of behaviour could be traced for the wave-particles. Their dual nature prevents that. Suppose, we can say about the electron-particle that at a given moment it is here and only 'here': but how can one say that about the electron-wave? A wave is not localized at one site—it is 'there' and 'here' simultaneously.

It is not accidental that our classically educated and classically limited imagination baulks at visualising the motion of the submicroscopic particles. The depths of matter are a world of lost pathways! But should not there be something to replace them since the motion itself has not been lost?

It is very difficult to surrender the classical concepts. It was no less difficult for the creators of the new understanding of nature than it is for us. In fact, it was more difficult for them since we do not

have any responsibility in the matter while it bore heavily on them. They paid for it with the price of deep inner confusion—the traditional approach to things was reluctant to give way but had to do so. For some veterans of the quantum revolution this inner confusion led to a life-long spiritual unrest.

De Broglie still experiences it, and he is still attempting to develop successfully his understanding of the psi waves on which he placed such great hopes half a century ago...

He recalled how he 'had been delighted by the wonderful work of Schrödinger'. Before publication Schrödinger made the friendly gesture of showing his papers to the creator of the concept of matter waves. But de Broglie never suggested dissolving particles in waves—it was precisely the wave-like nature of particles that was dear to him. And he did not take to the concept of the wave packets: 'this hypothesis seemed to be unsatisfactory'.

But what role did nature assign to the psi waves of Schrödinger in the motion of particles? Louis de Broglie developed a very elegant theory of that named somewhat poetically 'the theory of the pilot-wave'. Summarizing his studies when he was preparing for his sixtieth birthday he explained: 'I specially placed the particle into the continuous wave and assumed that the propagation of the wave carried the particle with it.' And he gave another, even more graphical description: 'The psi wave in a sense "shows the way" to the travelling particle.'

But what is 'the way': the sequence of the spatial points through which the wave passed or will be carrying with itself the particle? Had the attractive concept of the pilot-wave not deliberately restored to the submicroscopic particles quite definite paths? Thus, the definite paths forbidden for such particles again

emerged in the atomic scene under a wave-like mask.

Of course, de Broglie expected this objection and expressed his concept cautiously: he said only that the psi wave 'in a sense' showed the way to the particle. But the inescapable conclusion was that the 'sense' was classical. That was precisely what he wanted! He said to himself he desired to preserve a 'strictly determined motion' for particles. To translate from philosophical language, strictly determined equals classical.

He was complaining about insurmountable mathematical difficulties. He encountered them when he attempted to substantiate his theory in finer detail. But were the difficulties not insurmountable, because the very concept was not true to nature?

The first to understand or, at least, to express convincingly the meaning of the psi waves was Max Born back in summer of 1926.

3

414 - 54

One can well ask incredulously how that could be so. It was none other than Max Born who had identified the Heisenberg square tables as the matrices well known to mathematicians, and who continued to develop the apparatus of the matrix mechanics—the mechanics of particles and jumps—from the summer of 1925. It was he and his highly talented assistant whom Hilbert had made fun of quite recently, in the spring of 1926, for ignoring his good advice to seek for a wave equation for the matrices. And now it is to him that the credit for discovering the physical meaning of the psi waves goes!

The simplest thing to do is to shrug dismissively—still another irony of fate... But there were deeper

roots. The no-longer-young professor from Göttingen —he was forty-three—was no extremist. He did not share the biased preferences of his assistant. When Schrödinger's mechanics had appeared Max Born started to study the collisions between the submicroscopic particles using the wave equation and hoping for success.

In short, he rejected sectarian bias. His broad understanding was rewarded with a significant discovery—but first he had to listen to charges that he had 'betrayed the spirit of the matrix mechanics'. A charge of treason, no less! Of course, it was Heisenberg who made this allegation. Born added, recalling this episode: 'But soon he thought better of it and found a wonderful way to reconcile the corpuscular and wave pictures...' We shall talk about that below. But what was it in wave mechanics that tempted Max Born?

It was, primarily, the conventional and easily accessible mathematical apparatus—the equations, the continuity... everything as in classical physics... That was what won the hearts of everybody—even many classical diehards took a fancy to its mathematical conventionality. Some regarded it as a promise of an imminent return to the classical concepts. Of course, Born's 'treason' did not go that far—he did not want to sacrifice the particles and the quantum jumps to satisfy Schrödinger. Later he gave a funny explanation of that:

> 'That was because my institute and the institute of James Franck were located in the same building of Göttingen University. Each experiment conducted by Franck and his students on the collisions of electrons was to me new evidence of the corpuscular nature of the electron.'

Can one really believe that if the experimenters had worked in another building, a more distant one, the electron would have stopped seeming a particle to Max Born? Still the proximity of the experimenters was, probably, a significant psychological support to him in keeping the corpuscular concept of the electrons alive. Even in his short Nobel lecture almost thirty years later Max Born lyrically recalled how the Geiger counters had clicked recording the electrons, and how the traces in the fog in the Wilson chamber had visually demonstrated the electron tracks. That was clear evidence of their corpuscular nature. But how about the wave-like character?

Max Born told about that in the same lecture. He recalled how in 1925 he and James Franck noted a wave-like behaviour of the electrons passing through crystals—the diffraction by the sites of the crystal lattice! They instructed their young student Elsasser to study this phenomenon in more detail... Thus, the proximity of the experimenters stimulated the theorist to keep on thinking about the wave-like nature of the particles.

Not that Max Born was completely impartial—'...I regarded the collisions between particles as the scattering of waves...' That was why Heisenberg was angry with him in summer of 1926. But impartiality had won. That study had led Born to an understanding of the meaning of psi waves.

He had the advantage of a good and grateful memory. He did not forget one old constructive idea of Einstein's and it helped him.

In the same year, 1926, the quanta of light—the Einstein particles of light—were at last named. The physical chemist Lewis called them *photons*. The name became widely accepted. It was similar to the names of the submicroscopic particles—electron and

proton—and thus emphasized the corpuscular nature of the quanta. It was the corpuscular nature that had to be emphasized since their wave-like—electromagnetic—nature had never been questioned during the twenty years they had been known. Einstein suggested his concept of photons in 1905 and from the very beginning he had to answer one obvious question—if light consists of particles then what are electromagnetic waves about?

We have to repeat the answer here: the wavelengths or the frequencies of these waves described the energy of each quantum. But what about the crests and the depressions of the waves or the amplitudes? What were they describing if the brilliance—the intensity—of the light depended on them? The answer was simple and logical: the greater the brilliance, then the larger the number of the quanta, and the higher their density. This is what is described by the amplitude of the electromagnetic waves.

Now Max Born had to answer precisely the same question: what was described by the psi waves with their depressions and crests if the behaviour of the particles was associated with these waves? He remembered the Einstein idea and found the answer; on many later occasions Max Born gratefully remembered that debt.

It seems a usual sequence—each theorist has to remember what has been done in the field before. True, but one had to understand that Einstein's concept had been related to his problem, but here this was not obvious at all. For instance, Schrödinger's desire to regard the particles as some quanta built up of his psi waves could not be realized. No 'psions', parallel to photons, could exist. Therefore, Einstein's ideas on the role played by the crests and depressions of the electromagnetic waves had to be applied to a

case which had quite a different nature. Some extremely careful reasoning was needed for a successful application of these ideas though the questions looked very similar.

The simplest answer would be that the particle is at a given moment to be found at the site of the crest of the psi wave. But, according to Einstein, it was not necessarily so; for instance, was there no light at all at the site of the incline of the electromagnetic wave? No: the intensity of light was simply lower there but even there there were photons. Their number is smaller but they are there. Why could one not then assume that the electron could be at the slope of the psi wave? (Or any other submicroscopic particle whose behaviour is analysed.)

One is even tempted to think that there is more electron material at the crests of the psi wave and less electron material on the slopes of the wave. It is as if this electron matter is spread out all over the space where the psi wave describing the behaviour of the electron propagates—its density is higher at the crest and smaller on the slope. But then the electron as a particle disappears!

It was not accidental that Schrödinger succumbed to this temptation—his concept of the wave packets was similar to the concept of the electron spread out all over the space of the atom. 'I couldn't follow him in that', said Max Born.

Instead he followed Einstein who did not spread the quantum of light out over the electromagnetic wave since then the particles of light would be irrelevant.

No, the electron as a whole could be found where the psi wave reached a crest and where it followed a slope: but the chances of finding the electron increase as we approach the crest. There is no chance of

finding the electron only where the psi wave vanishes —there the probability of finding the electron is zero. The Schrödinger psi waves are the *probability waves*!

These immaterial waves describe the non-classical motion of the submicroscopic particles. It is as if they had replaced the strictly defined classical paths. Max Born theoretically substantiated it in the summer of 1926.

4

It was an event of primary importance for our good story. Surprisingly, it had practically no effect on many physicists at the time, particularly on those belonging to Niels Bohr's circle in Copenhagen. They had been convinced for a long time that the depths of matter were a world of probability laws. Niels Bohr himself had had this conviction from the time he had developed the quantum model of the atom in 1913.

The electron in the hydrogen atom could make any possible jumps along the steps of the energy ladder. The spectral lines demonstrated by their series (such as that of Balmer) that all types of allowed quantum jumps were taking place. Multitudes of atoms emit quanta simultaneously but each of them emits only one of the possible quanta. And if one line in the spectrum is more intense and the other is less intense this means that more atoms emit the first quantum and a smaller number of atoms emit the second quantum. In other words, the first kind of quantum jump has a higher probability than the second. The intensity of the spectral lines directly shows the varying probabilities of different kinds of jumps. The spectro-

scope in a laboratory operates as a bureau of statistics—analysing all the data it gives silent but visible reports to the physicist on the statistical frequency of different cases.

In short, there was nothing unexpected in Max Born's concept for Niels Bohr and his students. They were thinking on the same lines.

The probabilistic interpretation of the psi waves looked quite obvious even to the eighteen-year-old Lev Landau. Then in 1926 he was engaged in his first independent study in wave mechanics. Later, recalling the past and, in particular, his first study he said (to me) that Max Born, in fact, had found nothing new: 'Everybody thought that'.

That was, of course, an exaggeration. Neither Schrödinger nor de Broglie thought that, nor did many others who were concerned not only with the 'techniques of the quanta' but also 'with the philosophy of the quanta'. However, Landau's opinion is supported by many veterans who claim that the understanding of the quantum laws as the probability laws was already in the air.

Max Born said that 'it seemed almost self-evident' as if trying not to overestimate his own results. Yet it wasn't accidental that he said 'almost'.

By the end of 1926 Max Born received a short but very distressing letter, Einstein's reaction to the probabilistic interpretation of the laws of the new mechanics. His reaction was amazing...

He did not use physical arguments but called to witness his philosophical understanding of nature which he believed to be infallible: 'The quantum mechanics deserves high respect. But an inner voice tells me that still it is not what is needed.' He continued: 'This theory yields much but it hardly takes us clos-

er to the mystery of God. At any rate, I'm convinced that He doesn't play dice.' Thus, in his opinion there was no place in nature for random probability.

This seems to be a half-joking refutation with its reference to the inner voice; there are no formulas, no arguments... But still, it was too serious to be complacent about it. It was ironical that Max Born followed Einstein step by step faithfully but when he had reached the goal Einstein disowned him.

The probability concept of the world had started its quest...

5

The understanding of the second mystery—the strange multiplication of the matrices—could not be credited to a single person. But it was, perhaps, Niels Bohr who did more than anybody else to explain it.

The first response of Bohr to the unexpected formula AB is not equal to BA is not recorded anywhere—not in the interviews between the veterans and the historians, not in their memoirs, not in the writings of Bohr himself. But if he, as Heisenberg, Born, and Dirac, had felt consternation or confusion when he had first seen the formula in autumn of 1925 he would not have missed the chance to describe it, since he was fond of such psychological details of the past.

Apparently, it was true that he understood it immediately. And we cannot now ask him how he came to it—still another among many reasons to be sorry that in 1962 the historians came to him to talk about the Sturm und Drang period too late and his interview had been cut short at the year 1922...

Let us try to imagine how it happened.

Looking at the nonsensical formula he immediately understood that A and B could not be numbers. Whatever the order of the numbers the product of their multiplication is always the same—AB always equals BA. Things are different if A and B are not the observed quantities but the operations on the observed quantities. If the order of the operations of different kind is changed then the result need not be the same.

Say, the operation A is anaesthesia and the operation B is the extraction of a tooth; then the combination AB is tolerable while the reverse combination BA is unthinkable; no experiments are then needed to prove that $AB \neq BA$.

A most natural operation on the observable quantities in the submicroscopic world is their observation or, in other words, their measurement. But we can measure nothing in the unseen and soundless atomic world without receiving back from it an answering signal in response to our laboratory question. And a signal requires an expenditure of energy and time, in short, action. The smallest or the weakest of all the possible signals is the Planck quantum of action h. However small it is its magnitude has a real significance on the scale of the submicroscopic world, as we have repeatedly mentioned above.

The electron or the atom changes its state even after sending such a negligible signal. The measurement violates their being, each time differently. So can one be surprised that when two operations A and B are performed their order is by no means insignificant. This obvious fact had to be necessarily manifested in a true mechanics of the submicroscopic world. Hence the formula $AB \neq BA$.

Bohr for a long time had been thinking about the

measurements in the submicroscopic world. This purely technical problem of classical physics acquired new philosophical and theoretical aspects in quantum physics, because the submicroscopic world is extremely sensitive—like the princess in Andersen's fairy-tale who was bothered by a pea under the thickest of mattresses. The measurements cannot help affecting the measured quantity, and this fact introduces the yet unknown features into the very structure of our knowledge. Such are the consequences of penetration into the innermost depths of matter...

Bohr was gladdened rather than confused by the incommutativity of the matrix multiplication. Again one recalls an Andersen fairy-tale—an Ugly Duckling was transformed into a beautiful Swan. To be more prosaic: a nonsensical formula turned into a substantiation of Heisenberg's theory.

Later Heisenberg used to recall with a mixture of pride and embarrassment how he had been baffled by the multiplication of matrices on Heligoland and how he had consoled himself 'Fortunately, I don't need it, fortunately it is not very important.'

Of course, the fundamental laws of nature revealed the probabilistic character in the matrix version of the submicroscopic mechanics just as they did in the wave mechanics. To show this we can continue the comparison of the square matrices to the tournament tables.

For a score to be written into the table a game has to be played. Can we state that the score exists before the game? Before the game only a variety of possible scores could be predicted. Some of them were more probable and others were less probable. But nobody could give an absolute prediction, even computers which are allowed to make errors within an acceptable range.

Is that not exactly so in the mechanics of the observables? A game should be played—a measurement should be made to make an observable quantity into an observed value. In terms of the dialectical logic it is known as transformation of the possible into the actual. Before that, calculations yield nothing absolute.

It is tempting to think that the measured values, for instance, of the coordinate and the velocity of the electron, had really existed before the measurement. It is tempting but naive. There is no physical meaning in the belief. The simple question—how do you know it—has no answer.

Of course, the mechanics of the observables, as microphysics in general, leaves no room for doubts that the **electron exists before and irrespective of our observation** (otherwise, there would be nothing to discuss and nothing to measure). But quantum mechanics refuses to talk about the exact position of the electron without measurements.

Indignant refutation of this refusal is a thankless and futile occupation. Indeed, the electron is not a classical corpuscle but a wave-particle with all the resulting 'unpleasant' implications that we have discussed. (And it is irrelevant that the creator of the mechanics of observables also did not like this dual concept!)

6

Twenty years later in 1949, after the Second World War, a group of Soviet physicists headed by Valentin Fabrikant staged an elegant experiment.

Over the years a simple experiment had been repeatedly performed: an impenetrable screen with a

small hole; a photographic plate behind the screen; a straight beam of electrons is passed through the screen; the traces on the photographic plate are analysed. What were the results? Classical physics would predict the appearance of a black spot just opposite the hole in the screen; if the electrons were small balls nothing else could have happened. They would all hit the same spot within a small range.

Quantum physics predicted a different pattern, a much more interesting one. The electrons passing through the hole must reveal their wave-like behaviour. The plate must record the image that described the intersection of the electron wave with the plane of the photographic emulsion. The plate would be darkened where the wave crests intersected with it, and where the wave has a zero height there would be no darkening. Thus, there would be a black spot opposite the hole and alternating light and dark rings around it. This is precisely what was observed!

But even such impressive experimental results had not shaken the unbelievers or shielded the concept of the quantum-probabilistic world from the ordeals that began in 1926. And it was not only Einstein who doomed this concept to torments. Yet these proved to be a test of its strength.

Among many doubts there was the following very persistent one: was not the probabilistic pattern on the darkened photographic plate produced by the striking electrons the property of the electron beam (which contained an enormous number of electrons) rather than of each electron individually? Thus in 1949, the Soviet experimenters decided to conduct the experiment with a hole in the screen in a quite different way.

They released the electrons one by one, rather than in a beam; it was a marching file rather than a

crowd. They decided to let each electron pass the hole and hit the screen independently of the others: if the photographic plate revealed a wave pattern even under these conditions, then there would be no doubts that the properties of the electron beam were not significant.

The electrons appeared like water drops from a dripping tap. The interval between two electrons was thirty thousand times longer than the time needed for the electron to pass through the apparatus from the source to the photographic plate. Not one of the electrons 'knew anything' about the other electrons: it did not belong to a crowd and that could not influence its behaviour. The wave-like pattern of dark and light rings still appeared on the plate. Each electron could use--and did use!—only one of the possibilities to hit the plate not directly opposite the hole in the screen. But all the electrons together had used all the possibilities because their number was sufficiently large. It was indeed an elegant experiment. One wonders, however, that physicists wanted to perform it so many years after the quantum revolution had succeeded. Did it not mean that the revolution was continuing? Of course! The clashes of ideas and passions never stopped for long. Maybe, they will never stop altogether—we cannot get rid of the classical macroscopic character of our imagination. One can easily imagine how fierce these clashes were in the twenties, in the initial period of the quantum revolution!

Late in summer of 1926 Sommerfeld held a theoretical seminar in Munich. A report was given by Erwin Schrödinger who came from Zurich. Among the audience was Werner Heisenberg who was spending the remaining days of the summer vacation at his par-

ents' house. Thus, the creators of the two versions of the quantum mechanics accidentally met for the first time, face to face.

It was no great surprise that the middle-aged Wilhelm Wien, the director of the Institute of Experimental Physics, also came to the seminar. Though he was a dedicated opponent of the quantum innovations, his dislike did not include the wave theory of the Swiss guest.

Wien had formulated a well-known and in its time very significant equation of the pre-quantum theory of radiation. He now thought that Schrödinger was at last restoring the pre-quantum continuity to physics under the old motto 'Nature does not make jumps!' He felt an extreme dislike, however, for the concepts of the mechanics of particles and jumps invented by the former student from Munich, Heisenberg. In 1963 Schrödinger's widow Frau Annemarie recalled in her interview with the historians that generally the 'older people' had approved the wave concepts of her husband while 'the younger people' had not...

In Munich the young Heisenberg felt free to talk heatedly about the unwaranted confidence of Schrödinger in the wave packets, and he started to criticize him for his generalization of waves. Wien still remembered how scarcely three years ago this same university graduate had not been able to answer his question during the examination about the resolving power of the microscope(!) It was only through the intercession of Arnold Sommerfeld that the immature youth had been awarded the degree of Doctor of Philosophy. Now the ignorant young dog was criticizing a professor from Zurich as an equal. Forgetting his age the indignant Wien jumped from his seat and conscious of his position cried: 'Young man, you

have yet to learn physics and it would be better if you were good enough to resume your seat!'

He added that, of course, he understood the feelings of the poorly educated young man since the wave mechanics naturally put an end to such nonsense as the quantum jumps, but it was tactless to insist on the difficulties related to the wave packets: 'We do not doubt that Herr Schrödinger will overcome them in the nearest future!'

This episode was a starting point of a memorable event in our good story—the meeting of Schrödinger and Bohr in September of the same year, 1926.

Heisenberg tells in his memoirs how he was depressed when he returned home from the seminar and the same evening he wrote to Bohr about the unhappy turn of events of Sommerfeld's seminar. 'Apparently, it was my letter that made Bohr invite Schrödinger to spend a few days in Copenhagen in September. Schrödinger agreed and I hurried back to Denmark.'

As usual, Schrödinger emerged from the train at the Copenhagen station with a knapsack on his shoulders. That was his usual baggage for the Solvay Congresses, too. In Berlin he astonished his respectable colleagues by coming to deliver university lectures without a tie in tennis shoes and shirt-sleeves. In short, he did not feel an alien in the free atmosphere of Copenhagen. At the railway station immediately he got caught up in the dispute between Bohr and Heisenberg who both came to meet him.

A scientist with a knapsack on a railway platform... One could regard it as a symbol—'Physics on its way!' A year earlier Pauli and Bohr discussed the rotating electron at the stations of Hamburg and Berlin, Kronig and Pauli discussed it on the station

in Tubingen, while Pauli and Born discussed matrices in the train going from Göttingen to Hanover. It was a time when theorists felt that no discussion could be put off.

Bohr brought his guest from the station to his house. Though Schrödinger felt unwell and then fell quite ill for a few days the discussions once started continued for hours near his sick-bed. Some people even suggest that it was precisely these heated discussions that made the guest (who suffered from insomnia) take to his bed. Shrödinger and Bohr argued intensely while Heisenberg was predominantly silent. Clearly, he kept silent to prevent them attacking him and his one-sided approach.

As can be easily imagined, Schrödinger attacked the concept of wave-particles and the concept of the quantum jumps. His arguments were quite consistent logically—the concept of the wave-particles was inherently contradictory and therefore untenable, while the quantum jumps were nonsense since any motion was continuous.

How many times had Bohr heard these arguments. How many times he had repeated them to himself. This faultless logic had one weakness—it was based only on the classical experience accumulated through the centuries. According to Heisenberg, Bohr answered:

> 'What you are saying is absolutely correct. But it does not at all prove that there are no quantum jumps. It proves only that we can't imagine them, that the objective concepts of everyday life and the experiments of classical physics become inapplicable when we approach the description of the quantum discreteness. And we should not be surprised by

> that since we realize that the processes involved here do not appear directly in the sphere of our being.'

Schrödinger imprudently retorted that he was not interested in forming the human concepts of nature: 'I prefer leaving that to the philosophers'. This was precisely what Bohr was—a physicist-philosopher.

The years had not blunted the aspirations of his youth, to 'go deep into the essence of things'. It was this feature of his mind and willpower that made the professor from Copenhagen the leader of the quantum revolution. Rutherford had immediately appreciated these qualities. Though he used to be condescending to the pure theorists, Sir Ernest said about the quiet Dane: 'Bohr is something else...' He saw in Bohr a theorist who was concerned not with mathematical tricks but with the structure of nature and the structure of our knowledge of nature.

Bohr could not leave things to the philosophers as Schrödinger was prepared to do. Heisenberg was surprised how Bohr turned into 'an almost merciless fanatic' at the side of the sick man: early in the morning he used to waken Schrödinger who had bad nights and renew the seige of this 'wave fortress' thus abandoning his normally quiet ways. At last, Schrödinger had exhausted all the defensive arguments and cried out in despair:

> 'If these damned quantum jumps indeed are retained in physics I will not be able to forgive myself that I had something to do with the quantum theory!'

Bohr answered politely:

> 'But we are all extremely grateful to you for what you have done! Your wave mechanics

brought with itself such a mathematical clarity and simplicity that it proved to be an enormous step forward...'

Heisenberg who witnessed this conversation saw the merciless fanatic suddenly give way to the good-natured Bohr that he was accustomed to. Why? Because the dispute had ended. It was not resolved but really ended—the last phrase of Schrödinger meant that the polemic was not now about science but about the dramatic human situation when a man could not part with his prejudices... Of course, one could not answer oaths with rational arguments.

7

Heisenberg believed that it was a defeat of the wave heresy. Though Schrödinger did not give up the important point was that he was unable to defend himself. Did it not mean that physical truth was on the side of the matrix mechanics?

Little did he know that the time of his trials had now come and they would take not just a few days but several months. There would be also clashes of passions. His ordeal would end only in March of the next year, 1927, but the conclusion would be different: the summit of the Sturm und Drang period would be reached, a quest would be successfully completed. Meanwhile the fight with Bohr was imminent.

Much later when he was 'over sixty' he admitted in his long interview with the historians:

> 'I would always have the electron in mind as a small ball, as a sphere. Only I would say: "It may also be useful sometimes to call it a wave, but only as a way of talking, not as a reality" '

When he was young he also was not overly concerned with forming the human understanding of nature and was ready to 'leave it to the philosophers', at least at that time. But he knew well the opinion of Bohr and gave a perfect description of it in his memoirs:

> 'Bohr tried to take into account the simultaneous existence of both the corpuscular and wave pictures in everything. He believed that only both pictures could together provide a full description of the atomic processes.'

He added that he 'disliked this outlook'. He was not going to renounce his corpuscular heresy, just as Schrödinger was not going to renounce the wave heresy. Clearly, there could be no peace in the Copenhagen institute even after the professor from Zurich had left.

Heisenberg lived in the garret of the institute's building and Bohr lived in a cottage nearby. Frequently after a day's work in their offices they went to the bachelor's flat of the younger scientist and continued their discussions late into the night with a bottle or two of wine to stimulate them. Both were quite hardy men but the late nights told, even on them.

The only goal of the endless discussions was to understand how quantum mechanics could produce true results in spite of a strange illogicality quite inconsistent with classical physics. They had understood already the meaning of noncommutative multiplication and the probabilistic meaning of the psi waves but Bohr was not yet satisfied. He insisted that they had missed something fundamental, that they failed to grasp something of universal significance. When he criticized the dislike that his younger co-

worker expressed towards the equal significance of the concepts of waves and particles it was a reflection of his far-reaching quest. Their thinking and their concerns were not on the same scale in those discussions.

Heisenberg told the historian Kuhn in 1963, with a self-deprecatory smile (which was 37 years late, though), that in that discussion he had primarily wanted to establish the absolute predominance of his baby—the matrix mechanics. He placed all his hopes on the flexibility of its formulas:

> 'Mathematics is clever enough to do everything by itself without physicists' speculations.'

But Bohr was tormented by an urge to explain how nature could reconcile the concepts of particles and waves despite their total incompatibility. (That was how Heisenberg himself was describing Bohr's problem to the historian.)

Among other experimental observations it was the white traces of fog in the Wilson chamber that most frequently attracted their attention. Any day they could see these traces of charged particles in their laboratory installations, as Max Born did in the laboratories of Franck's institute.

Perhaps, it was the fog in the Highlands of Scotland that hinted to the Scotsman Charles Wilson, a friend of Rutherford's, how to design his chamber. This common natural phenomenon is due to supersaturation of the air with water vapour. Such vapour is always ready to condense in tiny drops of water if there is anything to condense on. The charged particles are as good centres of condensation as any, and fog is formed along their path.

The quiet scientist devoted to the study of fogs de-

signed his cloud chamber in 1912, the charged particles passing through it left a trail of tiny droplets which could be photographed. The white tracks left by electrons or ions in the Wilson chamber have the same origin as the white trails left by unseen aircraft high in the sky. But this familiar comparison could be made only a long time after Wilson had put forward his concept.

Everything looked so reasonable in the photographs taken in the cloud chamber but in fact it was all so bafflingly inexplicable. No wonder Bohr and Heisenberg could not sleep at night.

Thus, the white tracks of fog allowed the scientists to trace accurately the motion of the electron in time and space, did they not? They even made its path visible, did they not? In addition, when Pyotr Kapitsa placed the cloud chamber in a magnetic field in 1924 the paths of the heavy alpha particles could be seen to curve becoming similar to the parabolic path of a stone thrown parallel to the ground. The tracks of light electrons were transformed into circles in the magnetic field reminding one of the electron orbits in the atomic model of Rutherford and Bohr. All this could be seen with the naked eye.

But the matrix mechanics was based on the assumption that the orbits and any paths of electrons were unobservable. The wave mechanics did not question this assumption. So what could be done about such a glaring contradiction between the excellent theory and the excellent experiment?

Two leading theorists were asking each other plain questions and could not answer them. Heisenberg could not appreciate Bohr's ideas in full, especially his intuitive guess that they yet had to find some fundamental principle or law. They both dived into unknown waters but to different depths. Thus, they

were dissatisfied with each other and it was a more dramatic development in their relationship as in purely human terms they continued to like each other. But the stress of the prolonged and futile quest told on them both.

It would be hard to believe that their work was fruitless if Heisenberg had not explained:

> 'None of us could understand how to reconcile the mathematical language of quantum mechanics with such elementary phenomenon as the electron path in the cloud chamber... Our discussions frequently continued long past midnight, our strenuous efforts had not yielded any satisfactory results, after a few months we both were approaching the state of complete exhaustion and our nerves were stretched to the limit.'

Meanwhile the year 1926, full of decisive events in physics of submicroscopic world, gave way to the year 1927 which was destined to become the summit of our good story.

The ascent to the summit started in February when the desperate search in the dark led to a quarrel between Bohr and Heisenberg. They had planned a trip to Norway together hoping that cross-country skiing would cool their tempers and help them to find a way out. One night Bohr, however, cut short their discussions. Next morning he went North alone leaving Heisenberg behind. Heisenberg told to the historians later:

> 'He wanted to be alone, to think alone, and I think he was quite right.'

8

One can damn a bothersome opponent, aloud or in thoughts, but how can one get rid of a problem? It is the same as getting rid of oneself—a hopeless task.

The main advances in science are discovered by those people who are ~~in~~capable of self-alienation. Of course, for a search to be successful the seeker must be able to move at some distance from his ego: only after having performed this feat of self-alienation can he dissolve himself as a whole in his search. Yet it is this very act of dissolving that restores him to himself; indeed, he dissolves in what is the essence of his inner life. His ultimate strength is determined by what is effectively the lack of self-alienation. The skill of concentration has a dual meaning—it combines the capacity simultaneously to completely ignore one's ego and the capacity to withdraw fully into oneself.

It is high time now to start a new chapter and to head it 'The Main Chapter' but one does not want to make a break in the tale.

If somebody saw Bohr in Norway at that time he might think: here is a tallish man, slightly over forty, a good skier, stays apart, prefers solitude, maybe a country parson or a schoolteacher from a nearby town—may he enjoy his vacation!

If somebody saw Heisenberg strolling in a park close to the Institute in Copenhagen he might think: here is a skinny young clerk of twenty or thereabouts, chooses lonely spots, maybe just tired or has an unhappy affair—good luck to him.

But both were jumply thinking. It was as if the problems that had separated them continued to maintain a telepathic link across the sea and snowfalls between Norway and Denmark.

Heisenberg wrote in his memoirs:

> 'On the whole, I was glad that he left me alone in Copenhagen where now I had a chance to think quietly about these hopelessly complicated problems. I focussed my efforts on making a mathematical description of the electron path in the cloud chamber; rather soon I satisfied myself that the accompanying difficulties were quite insurmountable and I started thinking whether all our questions had been wrong. But where did we go astray?'

He kept on persuading himself that only observable quantities had a physical meaning and one night he recalled suddenly what Einstein had said to him a year before at a seminar in Berlin: 'Only the theory decides what we manage to observe!' It was only then that he finally understood the meaning of these words. Later he insisted that it had been as if he had seen in a flash a key to the closed door. He went out for a stroll in the park to think carefully about all that.

Bohr did not experience such instantaneous insight in the snowdrifts of Norway. There was no repetition of his old success when he had suddenly understood from the Balmer formula how the quantum jumps gave rise to the spectral lines. Indeed, it could not be repeated.

The discovery he was approaching was not a solution of a specific problem, though he was also thinking about such specific things as the particle paths in the cloud chamber. But they were for him just another knot in the confused tangle of human knowledge about the submicroscopic world. He was nurturing a justification for quantum physics that would totally clear it before the accusations of classical common sense, rather than on each of the separate

charges filed by conventional logic. The path of his thoughts looked like a gently curving ski route, but a route in the mountains where one could easily break one's neck or, failing that, the neck of his theory.

Unfortunately, he did not have time to tell anything about his trip to Norway to the historians. We know what Bohr brought from that trip only from Heisenberg's words. Of course, we cannot restore the course of Bohr's thoughts, we can only make guesses proceeding from our knowledge of his final results.

Bohr's starting point could well be the well-known statement of one of the creators of classical mechanics, the famous French mathematician Pierre Laplace: provide the physicist with the exact coordinates and velocities of all the bodies in the Universe at a given moment and the physicist will predict its state at any moment, as close or remote as desired!

That was an expression of belief in the omnipotence of the equations of classical mechanics. It was a justified belief at the time—these equations made it possible to trace the history of motion of any material body with a known mass from point to point, and from moment to moment. This can be done for the past or for the future just by taking the time with the minus or plus sign.

The future of any system of material points was determined absolutely from some moment by the values of the parameters known at that moment; these were called the *initial conditions*. And any material entities in the Universe were regarded as such systems, or systems of such systems.

This mechanics was an embodiment of the philosophy of absolute necessity, the absolute determinism of everything happening in the world without any exceptions. When somebody dies today or somebody is born tomorrow, it was predetermined by the motion

of atoms in the earliest time of primordial chaos. What we call the laws of Chance or the play of probabilities is just a result of our lack of knowledge about the details in the history of many events (why a deck of cards opens on an ace, why a meteorite falls here and not there?) But in principle any event can be traced to the very end.

Classical mechanics, as the philosophy of determinism, left no room for doubts in that respect, the possible and the actual were the same for it. But this was precisely what had long been questioned by Bohr and other scientists who recognized that the concept of quantum jumps reflected reality. The new development was not their doubt by itself, but that this doubt played a decisive part in finding justification for the strangeness of quantum mechanics.

In Bohr's thoughts physics was turning into a philosophy of nature. Otto Frisch recalls how in Copenhagen Bohr had used to cut short his opponent when the latter had embarked on a formal mathematical treatment:

> 'Now you are not thinking but making exercises in logic!'

Perhaps, he went alone that time to Norway, forsaking the companionship of a pure theorist, because the time for deeper thinking had come, the time to finally give up the philosophy of classical determinism, that is, absolute determinacy. He was essentially a physicist-philosopher and as such he was looking for a physical cause for the necessity of it—a cause for the lack of causality.

Sliding along the unblemished whiteness of Norwegian snow Bohr could think in the way he liked—shedding the prejudices instilled in all humans by the macroscopic experience of life, by human vision,

hearing and touch, and, finally, by the very language of human communication.

Meanwhile, Heisenberg strolled along frozen lanes of a Copenhagen park trying to understand what physicists really saw when they looked at the white tracks of charged particles on the photographs taken in the cloud chamber.

> 'We used to say easily that the electron path in the cloud chamber was observable but what we see in reality is, perhaps, something much more modest... Just a series of discrete and indistinct spatial cells which the electron has passed... Chains of individual droplets of moisture which are incomparably larger than the electron...'

It was an object lesson showing how right Einstein was—obviously, only theory could determine what our eyes saw. What did theory say about the foggy tracks of the electrons and the size of the particle-electron?

Take the smallest drop of moisture of which the electron track consists: its diameter is a thousandth of a millimetre (10^{-4} cm). The electron is smaller still by a factor of a billion (diameter 10^{-13} cm). If we enlarge the electron to the size of a fly (1 cm) then the droplet will be scaled up to the size of a planet. Imagine a fly inside a hollow sphere of the size of a planet. Imagine a fly inside a hollow sphere of the size of the Earth—that is what the electron looks like inside the droplet of fog. Where is it precisely and where does it travel? Obviously, the answers to these questions will be terribly vague. To ask about the electron path by looking at its track in the cloud chamber is the same as asking about the

path of the fly by looking at the motion of the Earth along its orbit.

Clearly different questions must be asked, different in principle:

> 'Can quantum mechanics describe the fact that the electron is only approximately present at a given point and travels at a given velocity only approximately, and how far can we reduce this approximateness?'

Classical mechanics would answer immediately that everything depended on the accuracy of measurements. Ideally, nothing should be approximate. Theoretically, measurements could be made absolutely precise. Formulas of classical mechanics yield absolutely accurate results.

Quantum mechanics had to be more cautious in its answers. The probabilistic behaviour of the electron prevented it from being too presumptuous. The quantum mechanics in the Heisenberg form writes down in its 'tournament tables' the numerous possibilities the electron has. It gives a less accurate but richer description of nature. It is the mechanics of the possible which has a varying probability of becoming actual, both in laboratory and in nature. Nothing that happens in the laboratory can contradict the laws of nature.

When Heisenberg tried to find a mathematical answer to this new question of approximateness he immediately put down the nonclassical formula $AB \neq BA$. He denoted by these symbols the ideal—most precise—measurements of the coordinate (A) and the velocity (B) of the electron. But now he started analyzing not quantities themselves but the possible approximateness (uncertainty) of their determination—ΔA and ΔB.

He decided to find out what happens to these uncertainties according to the laws of his mechanics—could they both vanish in the process of the electron motion?

He did not expect the classical answer 'They can and must'. If these uncertainties could vanish then the electron had a definite path and everything should be started again from the beginning. Soon he saw that the uncertainties could never vanish simultaneously. But he had to demonstrate mathematically how much they could be reduced, what was the highest accuracy that could be obtained in the measurements of the coordinate and the velocity at the same moment.

The limit dictated by nature had to be found.

Meanwhile, Bohr's thought left the classical ground and jumped across the sea of details to his first encroachment on the classical absolute determinacy—his discovery of the quantum jumps.

He used to say that a typical feature of the atomic processes was their integrity. We could not hold halfway an atom that emitted a quantum; there was no halfway and no halves of the quantum. The classical motto 'Nature does not make jumps' was replaced with the quantum motto 'All or nothing', either jump to a new stable state, or remain in the old state. He kept on repeating that the breakdown of continuity eliminated the possibility of giving a steadily causal description of the intra-atomic processes.

As the lonely skier had no opponent present he himself searched for clever objections:

'Granted, quantum jumps are untraceable. They can be likened to a jump across a ravine in pitch dark—the jumper was on one side, now he is on the other side but how he did it nobody could see. But

was not the trajectory of his jump strictly determined by the initial conditions of the jump? by the position of his body at the moment of the jump and its velocity? Unfortunately, we could not measure these initial conditions owing to darkness but our helplessness is irrelevant. The important thing is that the initial conditions did exist in reality. Then the classical equations can tell us everything we want to know about the trajectory of the jump. Why should we approach the quantum jumps in a different manner? Each of them has certain initial conditions, it is our concern to find them and nature is not to blame. If it is not practicable you may use the probability laws and speak in terms of probabilities of various types of the jump but do not draw any too far-reaching conclusions from that, do not say that the submicroscopic world permits no classical determinacy—you just have no right to say that.'

It was hard to object to such arguments. But the entire experience accumulated by submicroscopic physics demonstrated the need for objections. Bohr still had the nagging feeling that quantum mechanics lacked something without which it cannot convincingly disprove the classical objections.

These objections had one weak, even practically defenceless, point—the conviction that nature itself, in contrast to the helpless physicist, knew absolutely precisely the initial conditions of the quantum jumps. That looked like a religious dogma—classical physics said that it was so!

But classical physics said that time was absolute, and it proved to be relative.

Classical physics said that the physical velocity could be as large as desired, but it proved to have the upper limit—the velocity of light in vacuum.

Classical physics said that in nature the action could be as small as desired, but it proved to have the lower limit—the quantum of action.

Classical physics said that waves were only waves and particles were only particles, but...

The twentieth century brought many new developments and changed much in physical thinking. The classical dictate no longer could circumscribe the intuition of scientists looking for a true understanding of nature. But it was still required that the theory be noncontradictory.

While there remained exact initial conditions for the electron motion, at least in principle, no answer could be found to the classical objections. Classical physics would keep on insisting that paths do exist in the submicroscopic world and the jumps are not governed by the probabilistic laws.

But what if this belief is groundless? What if nature can do without definite initial conditions of motion? Then things would be quite different. Then Laplace's conviction—give me the exact coordinates and velocities for all existing material entities and I will predict the future of the Universe!—would lose its meaning. One cannot give what does not exist! The belief in an absolute determinacy would lose its last support in the depths of the matter.

Undoubtedly, a moment came when Bohr in Norway recalled the strange formula $AB \neq BA$, as Heisenberg did in Copenhagen. This formula indicated that the result of two measurements in the submicroscopic world depended on their order but it had also other implications.

If the order of operations is important the obvious conclusion is that they cannot be performed simultaneously. If they could, then their order would not have any significance—since simultaneity means the

absence of order, there is no 'before' and 'after' in it. Thus, a new queer feature of the submicroscopic world was revealed—it had observable quantities which could not be measured simultaneously.

Was not that the case for the coordinate and velocity of the electron? Yes, of course: the formula for noncommutativity of multiplication first appeared in quantum mechanics precisely for the measurement of the coordinate A and the measurement of the velocity B in the studies of Heisenberg, Born and Dirac.

But these are those very same initial conditions required by the classical mechanics for making its predictions. Now the structure of the submicroscopic world proves to be such that it forbids simultaneous measurement of them at any given moment. Bohr, of course, did not doubt that quantum mechanics correctly reflected the structure of this world.

If a theory is true its prohibitions are absolute; no laboratory tricks can overcome them. For instance, there is no way to overcome the law of conservation of energy—however hard you may try you will never achieve perpetual motion. So, however much you may want it you cannot determine exact initial conditions for the quantum jump. The laws of nature are categorical; you may be ignorant of them for the time being but you cannot violate them. Now the question was whether a law had really been revealed or it was just a clever speculation.

If it was the law then it was not peculiarities of quantum theory that prevented us from determining simultaneously the coordinate and velocity of the electron but nature itself. It means that nature in its depths does without absolute determinacy; it is *probabilistic*.

Though Bohr was sure of the validity of quantum mechanics his arguments however clever could not

replace a rigorously derived law. But that winter in Norway Bohr did not write anything on science, either letters or papers. He did not perform mathematical calculations. He just thought. He had a hunch that there was such a law.

Meanwhile Heisenberg completed his independent calculations in Copenhagen. He found the limit to which nature allowed us to reduce the uncertainties of the coordinate and the velocity of the electron.

Yes, the behaviour of a submicroscopic particle has a multitude of possibilities. This multitude cannot vanish, it cannot be reduced to something strictly determined. The simultaneous reduction of the uncertainties was limited again by the smallest possible action, the quantum of action h. Planck's 'mysterious ambassador from the world of reality' again made itself felt.

One morning in the second half of February 1927 the extremely excited Heisenberg put on paper the short formula relating two uncertainties:

$$\Delta A\,\Delta B \geqslant h \quad (\text{or } \Delta x\,\Delta p \geqslant h)$$

It meant that the product of uncertainties of the coordinate and the momentum (the velocity multiplied by mass) of a particle can be either equal to or higher than the quantum of action but can never be smaller than it.

It was immediately clear from this relation that if the uncertainty of the coordinate decreased, the uncertainty of the velocity increased; and, vice versa, the more definite the velocity the less definite the coordinate of the electron.

Now it became clear why the orbits in the planetary atom model were unobservable. We could determine separately either the coordinate or the velocity of

the electron-planet, but if we attempted to measure one of these quantities precisely then immediately the second one became quite indefinite.

The short relation also demonstrated another condition. Since the uncertainties appear in pairs they have a restraining influence on each other. Of course, the chance rules where numerous versions of behaviour are possible; but the relationship between the uncertainties mitigates the harsh dominance of chance. The rule of chance in the submicroscopic world is not wholly arbitrary.

As he had done in 1925 after his stay on Heligoland, Heisenberg decided to first of all tell his old pal Wolfgang Pauli about his new discovery (though 'old' sounds inappropriate here—both were still in their twenties). It was, perhaps, the longest letter Heisenberg had ever written and that Pauli had ever received—14 pages of scientific arguments (almost a completed paper for a scientific journal).

Heisenberg received the answer from Hamburg before the return of Bohr from Norway. Pauli was untypically exultant in his letter. He called the discovery made in a Copenhagen garret 'the dawn' and exclaimed 'Let there be light in quantum mechanics!' Heisenberg's discovery became known under the unprepossessing name of the 'uncertainty relation'.

After the physicists had better understood its fundamental significance they started calling it the uncertainty principle. They were joined by philosophers because this short relation easily, and with every right, made the jump from the province of quantum physics to the kingdom of the philosophy of nature.

It was precisely that fundamental law which Bohr had been groping for in his long quarrel with the classical determinism, and that he had almost reached in those days in Norway.

Need we tell what Bohr felt when he saw Heisenberg's formula upon his return to the institute. The situation was both beautiful and dramatic. His former assistant the Swedish physicist Oscar Klein told the historians:

> 'Bohr was genuinely delighted by this wonderful formula. At the same time he was somewhat frustrated, perhaps because he had thought on the same lines but had not put it into the final form.'

Was not it 'wonderfully wonderful' that both, Bohr and Heisenberg, had by following different paths independently found what they had failed to find together? It can be very well explained psychologically.

They say that truth is born of discussions. In effect, that can be said about Bohr and Heisenberg; without their long seemingly hopeless discussions lasting several months neither of them could have reached the solution. But the contrary statement is also true—the truth dies in discussions, it can be simply smothered by them. The disputes continuously disturb the concentration of each of the opponents. In addition to mutual assistance, discussions produce a measure of mutual friction and hindrance. A Spanish saying warns that 'Two together cannot see a spirit.' Theoretical discoveries are akin to spirits.

The famous British philosopher and mathematician Bertrand Russell obviously knew what he was talking about when he said: 'Culture would be impossible without a capacity for mental solitude.' The outstanding Russian geochemist Vladimir Vernadsky, a thinker of unusually wide scope, wrote to his fellow members in the USSR Academy of Sciences sometime in that period, in the middle of the twenties:

> 'The entire history of science proves at each step the ultimate rightness of a lonely scien-

tist who sees what others have been incapable of seeing and appreciating in time.'

The emergence of the uncertainty relation is one of the best pages in the history of the quantum revolution. It can be seen as a parable that is good for all times. In our time of mammoth institutions and crowded laboratories many are convinced that it is only joint research efforts that can lead to success. They are mistaken. Joint efforts, of course, stimulate research work; nobody valued it higher than the scientists belonging to the Copenhagen school of Bohr or the Moscow school of Landau. But one should be able to draw away into mental solitude. Perhaps, it is the only way out of a blind alley—to go it alone. It was not without reason that Rutherford did not allow research workers to linger at the laboratory after 6 p.m.: 'You must not work all the time,' he would say, 'you should leave some time for thinking.' He knew how to see spirits.

Bohr saw two spirits during his Norway trip. After he had glimpsed the outlines of the uncertainty relation he came to what later became known as the principle of complementarity. Heisenberg said that Bohr had brought it from Norway.

9

What made Heisenberg say that? In fact, Niels Bohr started talking about the principle of complementarity or the theory of complementarity only in the autumn of 1927—and he had returned from Norway at the end of February. The explanation is that, as usual, the concept had emerged earlier than a name was found for it.

Bohr was not only delighted with what Heisenberg had written in his new paper on the uncertainty relation: he made some criticism, too. Yes, each new step in the Sturm und Drang period was closely analyzed and criticized; each such step had to be substantiated beyond any doubts. The stakes in the game were too high—the general acceptance of the new physical thinking. The younger scientist felt it no less acutely than his teacher. He told the historian Kuhn: 'I never sent a paper away before I knew that Bohr would agree to it.'

Bohr immediately saw some slips in the derivation of the wonderful relation: these did not affect the result but cast doubts on its rigorousness and necessity. The slips were caused by Heisenberg's continuing neglect of the wave-like nature of particles:

'I wanted to do everything from matrix mechanics and therefore I did not like to think about wave theory at this point.'

But he needed wave theory, for instance, for performing mentally the perfect experiment on the most precise determination of the coordinate and velocity of the electron. It had to demonstrate that uncertainties could not be eliminated even in the perfect experiment.

Imagine a supersensitive microscope. Of course, it is not practicable but in theory we can illuminate the electron with rays with the shortest possible wavelength. The rays pinpoint the electron and determine its position. The accuracy of determination increases when we decrease the wavelength of the illuminating rays. Even the X-rays have too long a wavelength for pinpointing the electron; their wavelength is comparable to the diameter of the hydrogen atom, and the electron is smaller by a factor of tens of thousands. You ask for a friend's address and get the

answer 'Somewhere within the city bounds'. Such is the precision of X-ray measurement if your friend is the electron.

Theoretically we can use increasingly shorter waves —for instance gamma rays emitted by radioactive elements—and obtain increasing accuracy of measurement. The uncertainty of the coordinate determination will decrease steadily. But when can the gamma microscope finally give us the absolutely precise address of the electron? Clearly, this can be done only with rays of zero wavelength. Yet no such rays exist. Moreover, in terms of quantum physics gamma rays are the fluxes of photons with a very high frequency of the electromagnetic field. The shorter the wavelength, the higher the frequency, the higher the energy of the photons. If the wavelength is zero the frequency is infinite. Such a photon would have infinite energy and hence infinite mass. That is physically meaningless.

In short, even the perfect experiment could not provide for absolute accuracy of measurement of the electron coordinate (there would be practical complications, too, but this is irrelevant here). Yet if something is impossible in the perfect laboratory then it is impossible in nature, too; physicists play their games only according to nature's rules.

So where does this lead us? The gamma microscope still theoretically allows us to decrease steadily the uncertainty in the measurement of the coordinate (though not to zero). But what happens to the second uncertainty, that of the velocity determination? The shorter the wavelength of the probing rays the higher the energy of the quanta comprising it. The higher the energy the greater the disturbance they produce in the electron motion; the electron collides with the photon at the moment its coordinate is measured, and

its velocity is changed unpredictably. Thus, the uncertainty of the electron velocity increases with the increasing precision in the determination of the electron coordinate.

This is what the uncertainty relation shows.

Clearly, the mathematical treatment of this theoretical experiment with the gamma microscope had to employ both the corpuscular and the wave-like features of particles. But Heisenberg did not like to think about wave theory. He had known beforehand that Bohr would not approve: 'I felt that Bohr would be displeased with my interpretation of the problem ...'

The scene that followed might be described as fiction had it not been described by Heisenberg himself:

> 'Bohr tried to explain that it was not right... I remember that it ended with my breaking out in tears because I just could not stand this pressure from Bohr. So it was very disagreeable.'

So that was how it went. Could Bohr's assistant Kramers have suspected when he had joked about the quantum victories ('first joys then tears') that his joke would some day be realized in the real-life tears of one of the most prominent quantum heroes? The ideas and passion were quite inseparable till the very end of the story.

Apparently, it was during those interminable hours under pressure from Bohr that Heisenberg heard the word 'complementarity' from him and realized what new understanding Bohr had brought back from Norway.

10

The head of the Copenhagen school kept on thinking about the pairs of the observables which for some reason could not be simultaneously determined. Indeed, what was the reason?

In fact, the anwser did exist for the coordinate-velocity pair—it was given by the particle-wave duality. But was such an answer sufficient? His imagination was tormented by its inability to deal with this concept; and his mind was tormented by its incapacity to reconcile logically the inconsistent concepts of wave and particle.

The basic mystery was why it was only the combination of these inconsistent concepts that gave a complete description of the nonclassical properties of submicroscopic reality. True knowledge was bought at the price of nonsense—the inconsistent combination. Why had one to pay such a price? For the umpteenth time Bohr came to the only possible explanation for the inevitability of such nonsense: physics was forced to employ the language of the macroscopic world to describe the submicroscopic world.

But did it really have to? Could it not learn to talk about atoms, electrons or quanta in their own nonclassical language? Then no inconsistencies or paradoxes would appear. One could learn the language in the laboratory by posing questions before the submicroscopic world and listening to its answers; the questions could be repeated as many times as was needed for understanding the answers. A good idea, was it not?

Of course, the idea was good. Yet the only way to listen to the answers of the submicroscopic world was to use the macroscopic language—the traces on the oscillograph screens, the particle tracks on the photographic plates and film, the deflections of the

instrument indicators... The events in the submicroscopic world must generate macroscopic information in the laboratory apparatus to be registered even with the finest experimental techniques. Otherwise, how could the physicist learn about them? All the instruments and apparatus we operate are necessarily on the macroscopic scale. Any new knowledge must, also necessarily, be described in the macroscopic language.

Twenty years later Niels Bohr lucidly explained to the readers of the magazine *Dialectics* who were well versed in philosophy:

> 'The term "experiment" can in fact be applied only to those activities for which we can tell others what we have done and what we have learned as a result.'

All this could be told only in the conventional language steaming from macroscopic human experience, which includes the elaborate vocabulary of classical physics—physics that describes the macroscopic world.

Oge Petersen, one of the last of Bohr's collaborators, incidentally—it was he who helped the historians in November 1962 to interview his seventy-seven-year-old teacher—recalled later Bohr's facetious speculations:

> 'Of course, it may happen that when the electronic computers start talking in a few thousand years time their language will be quite different from ours, and they will think us crazy because they will not be able to communicate with us. But it is not our problem ...'

In those solitary days in Norway Bohr felt the

burden of 'our problems' especially keenly. He thought about their fundamental features in the silent hours of cross-country skiing. It was not the first year that he pondered on these problems and now he had approached the principal questions... As before, we are unable to reconstruct his way of thinking but his ideas suggest the following reasoning.

Intelligent inhabitants of the submicroscopic world, if they existed, would also think physicists crazy if they appeared 'in a few thousand years' and started talking about some absurd 'particle-waves' and similar queer things. But physicists really have nothing to replace the classical concepts with.

Nothing would be changed essentially even if a new term was introduced for these entities of dual nature, say, wavicles. One would always have to bear in mind the interpretation of this new name for the particle-waves. There would be a new dictionary entry: 'Wavicles—submicroscopic entities possessing wave-like and particle-like properties'. The problem would return by the back door.

Now, imagine that a miracle has happened—physicists have found a comprehensible vocabulary of the submicroscopic world, free of inconsistencies. The impossible has happened! But it would not belittle another logical and linguistic miracle—and one which really happened at that—which consisted in the finding that the queer behaviour of the submicroscopic world governed by nonclassical laws could be successfully described in terms of classical concepts, even quite inconsistent ones!

Does this mean that the classical language retains its power even in the submicroscopic world? Before answering this question one must recall that a language includes not only words but also grammar, that is, the laws governing the use of words.

Of necessity, we must retain the vocabulary—particle, wave, coordinate, velocity, continuity, jumps, determinacy, probability—we cannot shed these concepts. But the grammar of classical physics is no longer applicable; we have on the one hand to reconcile the irreconcilable (like wave and particle), and on the other hand not to combine the fully consistent (like velocity and coordinate).

One can clearly distinguish new grammatical features alien to the macroscopic world. Though the words are conventional the description built up using them becomes unconventional, nonclassical. Then does not the old vocabulary plus a new grammar constitute the eagerly-sought language of the depths of matter, the one that the physicists try to pick up in their laboratories when asking nature searching questions?

In Norway Bohr recalled the disputes he had had with Schrödinger in September and realized that the wave heresy was born as a protest against the new grammar; it could, however, look like a protest against the undesirable old words, which claimed that the question could be settled by eliminating 'particles' from nature's vocabulary. The protest had a hidden meaning: if the undesirable words are removed from the vocabulary there will be no need to reconcile the irreconcilable—waves and particles, continuity and discreteness, probability and determinacy... The new grammar will not be needed. The corpuscular heresy of Heisenberg was essentially also a protest against the new grammar though on the surface it also seemed a protest against objectionable words. Both creators of the mechanics of the submicroscopic world craved for elimination of the opposites.

Bohr realized that no elimination was needed. It

was not against nature that inconsistent properties should have equal rights. Physicists had to recognize their co-existence: they were not in conflict, they really existed side by side. That was how Bohr's thoughts ran.

Indeed, had one any right to say that the wave-like nature of the electron was in conflict with its corpuscular nature? Can one state that the coordinate and the velocity contradict each other since they cannot be determined simultaneously? This has long been known in philosophy as the conflict of opposites; but the physical mechanism of this conflict is such that it could not have been revealed before—the opposites do not appear together in the scene of physical interaction, there is no struggle between them and no victors or losers.

There are no feasible experiments in which light or the electron could demonstrate both their inconsistent classical features in one event. They reveal either wave-like features or corpuscular features. In the first instance physicists observe the interference or superposition of waves and in the second instance it is the release of electrons under photon bombardment. Other examples could be cited.

There is nothing mysterious in this experimental duality. One must just bear in mind continuously that submicroscopic physics deals with objects of a dual nature. The experimental work with them is somewhat similar to using binoculars—one cannot look into the eyepieces from both sides simultaneously. They should be reversed, the observations must be separate. And this property of binoculars does not contradict nature.

We see a 'diminished' world looking into binoculars from one end and a 'magnified' world from the other end, and both images are equally real. The con-

clusion is that what we see depends on the method of observation. It is not the existence of the world that depends on the observation—the world is not even aware of the physicists' troubles!—but on that depends what and how we see in it. (If a mole looked into the binoculars from either end it would see nothing at all—it has no capacity to use instruments.)

The separate experiments in quantum physics are not in any conflict with nature, either. Such experiments are continuously staged by nature itself.

The blue sky we see above us is produced by the scattering of sunlight by molecules of atmospheric gases. One can fantasize that the Sun measures the positions of the air molecules by bombarding them with photons and thus determining their coordinates more or less accurately. Of course, no records are kept but that is irrelevant. The hydrogen atom in the stationary state is also a result of an accidental experiment. The electron conducted an experiment for measuring the attractive force of the proton and was caught by an unseen chain.

In some natural events the submicroscopic entities behave as particles and in others as waves. But what are they in reality? The answer is obvious—they are both.

Yet one recalls here the serious advice of Lev Landau not to overuse the term 'particle-waves' even in a popular account of the concepts of quantum mechanics: 'It is futile'. And he reiterated what he had said, perhaps for the first time at the Moscow meeting commemorating Planck's centenary (1958), that by '... studying nature man can overtake his imagination, he can discover and understand what he is even unable to imagine'.

Possibly, Landau indeed did not need the assistance of imagination in his studies. But how hard it is for

us! The dual concept of the particle-wave helps our imagination at least to get used to the strangeness of the submicroscopic world just *because* we cannot imagine it, while the wave and the particle are individually quite imaginable. This will help us also to understand why this strangeness makes scientists override their imagination. Was this not the reason why Niels Bohr so highly valued the concept of the particle-wave from 1925 onwards? Once Landau felt free to use this 'futile' concept himself in an interview about his own theoretical work: when told about Bohr's preference he retorted, laughing, 'Quod licet Jovi non licet bovi' (What is permitted to Jupiter is not permitted to the ox).

The strangeness of the grammar of the submicroscopic world lies precisely in its acknowledgement that the classically incompatible concepts or images are given by nature the right to complement, rather than exclude each other: the vice of incompatibility taken to the extreme of total conflict is transformed into the virtue of *complementarity*—such were Bohr's views when he returned from Norway in February 1927, and this is why he unrelentingly criticized Heisenberg's sins.

Soon Heisenberg's tears dried. He did not quarrel with Bohr but took his critical remarks into account and made the necessary changes. Perhaps it was at that time or a little later when Heisenberg understood, as Bohr had done, that the uncertainty relation was a manifestation of a more general principle of complementarity which had not yet been formulated in full.

The wonderful mathematical formula relating uncertainties was clearly indicating the irremovable limitations on simultaneous determination of the incompatible properties. To make a full description of

the variable probabilistic submicroscopic reality we have to consider the incompatible pictures of it as being *complementary* to one another. Otherwise, we would not be able to understand the whole.

From that time, and to the end of his life, Niels Bohr kept on convincing people working in other fields of science and the arts—biology, psychology, languages, the history of culture—that the principle of complementarity could give them guidance, too. He thought the principle was of a general philosophical significance and over time his ideas gained increasing acceptance. But that is another story.

When shortly before his death an assistant asked what proportion of his entire research work was devoted to the physical problems of knowledge his answer was: 'In a sense, it was my life-work!'

In a deep-hidden sense it was the life of quantum physics itself in the Sturm und Drang period, when the quantum probabilistic picture of the world was born in a clash of passions...

Concluding Chapter

There Is No End

> I describe characters, situations, details and features with the sole ultimate goal—to represent reality... as an image governed by choice and freedom, as a certain version among other versions.
>
> Boris Pasternak (20th century)

1

Almost all the leading participants of our good story met in the late autumn of 1927 as if to provide the story with an impressive finale.

The occasion was the Fifth Solvay Congress. The thirty two participants included Antoon Lorentz and Max Planck, Albert Einstein and Paul Ehrenfest, Niels Bohr and Max Born, Louis de Broglie and Er-

win Schrödinger, William Bragg and Peter Debye, Werner Heisenberg and Wolfgang Pauli, Paul Dirac and ... but enough names. It was perhaps the most representative meeting of physicists belonging to different generations—from seventy- to twenty-year-olds—in the Sturm und Drang period.

One recalls how Lorentz had stated the problem of developing the mechanics of the submicroscopic world at the First Solvay Congress, when the old men present had then been in their prime and those now in their twenties had been mere boys. Sixteen years had passed and the desired mechanics had been developed; Lorentz was again the chairman of the congress; it would be natural for him to praise gratefully those who had created the mechanics. However no words of praise were forthcoming—this mechanics proved to be undesirable for the great classical physicist. In the words of Heisenberg, it had to undergo a 'baptism of fire' during the fifth Solvay Congress.

Lorentz, the eldest of the participants, put his objections against it more clearly and concisely than other scientists holding the same views:

> 'I wish to have a quite definite picture of any phenomenon. An electron for me is a particle which is at a given point in space at a given moment in time. If the electron collides with an atom, penetrates it and after numerous adventures leaves it then I visualize a certain line along which the electron travelled in the atom.'

What could the younger scientist say to the venerable classicist? The very object of the dispute was missing. It was simply a return to the very beginning of their quest, to their own, laborious and futile, efforts to find this 'certain line' experimentally

and to describe it with an equation. Lorentz did not refute quantum mechanics, he just totally rejected it. Did the participants of the congress not sense that it was a tragic emotion deep inside that prompted his actions?

The creator of the classical theory of electrons, he apparently realized the inevitability of a new physical understanding of the world. Otherwise, he would not have said to Ioffe:

> 'I have lost the conviction that my research was leading to the objective truth and I do not know what I have lived for; my only regret is that I did not die five years back when everything still seemed clear to me.'

Few people could dare make such an admission: the great physicist had a great spirit. He proved that the drama of ideas involved a drama of human beings.

Einstein was the first to speak of the *drama of ideas*. It was on a later occasion and he had in mind not only quantum physics. But he could not help remembering the fifth Solvay Congress as a realization of this drama in which he was the main character. He played that part because, in contrast to Lorentz, he did not just reject the probabilistic quantum description of the world, he tried to refute its inevitability.

2

The congress took place in Brussels from October 24th to 29th.

Almost a year has passed since Einstein had written ironically and yet deeply seriously that God did

not 'play dice'. That had been in a private letter to Max Born but now in Brussels he said it publicly. It became his motto—lucid expression of his inviolable faith in classical determinacy, the good old determinacy which was absolute and left no room for choosing between probabilities—for tossing dice...

Everybody—not only supporters but opponents, too—understood that his position was firm. Heisenberg later recalled:

> '"God does not play dice" was his inviolable principle, one that he would not allow anybody to question.'

But maybe Einstein who worked alone just did not know that the uncertainty relation had been found in nature in the recent months? Of course: he must have made that statement when the principle of uncertainty was still unknown, and if he had learned of it he would have sighed sadly and meekly admitted his mistake (he was good at that!)

But in fact he knew perfectly well that a new fundamental law had appeared in quantum mechanics. Niels Bohr had sent him the proofs of the history-making paper by Heisenberg in April of 1927 (half a year before the congress in Brussels) with his highly approving commentary. Einstein had not answered Bohr at the time and now it was clear why...

Every day in Brussels was filled with discussions: after sessions scientists talked in the hotel restaurant, in parks, on the streets. These discussions were not reported in papers or in the news bulletins over the radio. Photographers and reporters did not flock to the battlefield. Nevertheless, it was a battle whose consequences were immeasurable. It was a major, if not decisive, battle of ideas in the vast field where the atomic age had to grow and mature.

What scoops the newspapermen had missed! But it was not their fault—they did not know the language of the dispute and could not understand its significance. The islanders in the Philippines have no common language, they talk in 80 languages one of which is used by just 26 people. This is what the language situation in quantum physics was like in 1927.

Now looking back we understand that the battle raging in Brussels for a few days was basically an unprecedented combat of two forces. Leon Rosenfeld, a Belgian historian of physics and theorist who had worked with Bohr for some time, compared it to a boxing match between two champions. But, maybe, a better parallel would be the Match of the Century between two great chess players—each day a new game, each game adjourned, careful analysis at night to find some brilliant and unexpected move because that was the only way to win. Each time Einstein made the first move and planned the offensive. Bohr had to choose the defensive strategy.

When Einstein made his declaration about a God who did not play dice those present noticed a victorious gleam in his eyes. Heisenberg recalls Bohr's rejoinder: 'Indeed, it is no business of ours to tell God how he must govern this world!'

Twenty two years later in a paper commemorating the 70th birthday of Einstein Bohr recounted his answer in a duller though subtler form:

> '...I answered that even the thinkers of antiquity had advocated an extreme caution in attributing to Providence the properties expressed in terms of everyday experience'.

He meant that physicists in their discussions of

the submicroscopic world had to be extremely careful in using the classical language stemming from our macroscopic experience.

Thus their dispute started with the exchange of aphorisms. That was a short exploratory game, the only one that ended in a draw, it did not really contain any physics. Then the time of physical arguments came. Einstein had the advantage of having prepared some of them beforehand. His guiding idea was quite clear.

He knew that the derivation of the uncertainty relation could not be refuted. It was derived from the fundamentals of quantum mechanics and reliably substantiated in imaginary experiment with a supersensitive microscope. But if the uncertainty relation was true then all was lost—the classical absolute determinacy was forever expelled from the physical picture of nature. Since this could not be allowed to happen—his philosophy of nature was all set against it—a system of arguments refuting it had to be found. The arguments, of course, must be physical. No other arguments—philosophical, religious, or psychological—could be used in discussing the laws of nature. Where could he find such arguments?

The answer was unexpected—he tried to find the arguments in quantum mechanics itself. The uncertainty relation had been derived perfectly well from the fundamentals of quantum mechanics, but was it a fact that these fundamentals were perfect, that is, complete in themselves? Einstein said, no, they are clearly incomplete, they lack something extremely significant. He did not know what it was that was lacking, he just placed his hope in the future.

His belief in the future was so strong that he immediately felt free to attempt a refutation of the uncertainty relation. In other words, he refused to

recognize it as a genuine physical law. He was convinced he could easily demonstrate that it was not satisfied.

But it proved to be not so easy. Einstein started to invent various intricate paradoxical cases—imaginary experiments—in which the uncertainties inevitably reduced to zero; they vanished as if the action quantum *h* had never existed. The certainty principle triumphed—at any rate, that was what Einstein thought. He was sure his arguments were irresistible. Hence the victorious gleam in his eyes.

But the gleam did not stay there for long: by the end of the first day Bohr had taken apart the imaginary experiment suggested by Einstein in the morning as a watch maker takes apart a clock mechanism of unknown origin and demonstrates that it does not work because of this and that faulty part... Now the gleam was in Bohr's eyes.

Yet not for long... But let us hear what the most interested witness, Werner Heisenberg, has to say:

> 'The discussions usually started in the early morning when at breakfast Einstein suggested a new imaginary experiment... Naturally, we immediately started analyzing it... As a rule the same evening at supper Niels Bohr successfully demonstrated to Einstein that even this newest conjecture of his could not shake the uncertainty relation. Einstein started to fret: but the next morning at breakfast he had ready one more imaginary experiment, more intricate than the previous one, which he believed would at least irrefutably demonstrate the invalidity of the principle of uncertainty. But by the evening of the same day this attempt proved no more successful than previous ones...'

Thus Bohr won game after game; his watchful analysis revealed each time some slight error in the current paradox of Einstein, demonstrating that the uncertainties could not be eliminated. Quantum mechanics won each game with the most powerful of possible opponents (if we do not count the draw in the starting exchange of witticisms on God and Providence). It could not be otherwise: nature was on the side of Bohr—on the side of quantum physics.

Paul Ehrenfest, a close friend of Einstein, said bitterly to him during this discussion:

> 'I am ashamed of you, Einstein; you attack the new quantum theory in just the same way as your enemies attacked the relativity theory!'

How right Bernard Shaw was when he said that the only lesson of history is that nobody learns from it. Even Einstein, great man that he was, could not become an exception to this eternal rule.

3

One day at the congress the participants gathered around the blackboard where Ehrenfest had drawn a cartoon. Everybody smiled understandingly as they looked at the half-built tower of Babel and the quotation from the Book of Genesis 'The Lord there made a babble of the language of all the world'. Thus Ehrenfest reminded scientists that this great construction project of the past had not been completed because everybody had begun talking in different languages. It was his call to the scientists if not to agree then at least to try to understand each other.

It was not only Einstein and Bohr who were locked in a bitter dispute. De Broglie insisted on his theory of the pilot wave, Schrödinger on the supremacy

of waves, and Pauli rejected all kinds of models. And the first did not agree with the second, or the second with the third, or the third with the first and the second, and so it went on... This confusion of tongues at the congress was a forewarning or a rehearsal of what was awaiting quantum mechanics in the decades that loomed ahead.

Fortunately, the framework of the tower had been completed by the autumn of 1927 by the scientists from different countries. Nothing could demolish it now, neither the controversy between the builders nor the seemingly impressive attacks by the old-school diehards. Remarkably, it was its most forceful opponent Einstein (definitely not of the old school) who unwittingly helped to strengthen it, first with his quantum ideas and then with his unyielding opposition.

Yes, it was his opposition that contributed to establishing quantum mechanics. In 1963, thirty six years after the Fifth Solvay Congress, the old veteran Werner Heisenberg explained to the historians that the outcome of the dispute between Bohr and Einstein gave rise to the feeling that a turning point had been passed in the development of quantum physics:

> 'I would say that a change had taken place which now I could only express in terms of lawsuits. That is "The burden of proof is reversed". The burden of proof suddenly went to the Wien people and so on because the rumour spread that there was a group in Copenhagen which can answer every question about experiments... So if you want to do anything against this view you have to disprove them. And the rumour also spread that nobody so

far has been able to disprove them, not even Einstein, who does not believe it... Einstein had not been able to disprove these people at the long conference in Brussels... Now there were a few people from Copenhagen who told the younger generation "You do this and you do that... now that this is correct and you can go ahead".'

Now 'this is correct' (the case is proven)! At this distance in time we can hardly appreciate the relief felt by the veterans of the quantum revolution after all that psychological strain. They had not yet thought of themselves in proud and somewhat grand terms as victors in the revolution—they just knew they had worked damned hard. They had reached the summit and stood firmly on it; the burden was lifted from their shoulders.

4

Here is where our good story ends—at the very summit of the crowning year in the Sturm und Drang period.

But does it really end? Of course not, it just breaks off... And as in any good story one is tempted to ask what happened next. If there was a summit was it followed by a descent?

But there are no descents in our knowledge of nature. Time steadily moves forward and so does science, not as monotonously as time but in jumps or steps. These steps can be small or large but they never descend—knowledge cannot diminish, it can only increase. Understanding is accumulated but never

depleted. In the periods of scientific revolutions the steps are steep and therefore difficult to climb. Knowledge grows in leaps and is therefore difficult to follow.

Then what is a summit on such a never-descending step-wise line? It is merely a roomy clearing before a new ascent whose outlines in the surrounding mist are still indistinct. One gets a false impression that one cannot go any higher. But it is just the steepness of the climb that creates for a time the illusion that a summit has been conquered. In fact, no such summit exists...

The most dramatic events of the Sturm und Drang period had happened by 1927. But even in the quite arbitrary time limits of the archive of the quantum revolution this period had five more years to run up to 1932.

If anything, the work became even more intense. More and more scientists came to till the newly opened fields. The number of papers published was counted not in dozens as before but in hundreds and then in thousands.

At the turn of the thirties more outstanding scientists were attracted to fundamental studies. Among them were many famous Soviet scientists—in addition to Sergei Vavilov, Pyotr Kapitsa and Lev Landau whom we have already mentioned, the following names feature prominently in this history: Leonid Mandelshtam, Igor Tamm, Vladimir Fok, Yakov Frenkel. The leading parts in the future nuclear projects in the USSR were to be played by Igor Kurchatov, Anatoly Aleksandrov, Lev Artsimovich, Yakov Zeldovich, Isaac Kikoin, Georgiy Flerov... and all of them started their careers in the Sturm und Drang period.

In that period theorists and experimenters snatched

eagerly for the latest issues of physical journals looking forward to some exciting news in science. Their hopes were always gratified: each month brought something remarkable, each year left its indelible trace in history.

In short, our good story had a magnificent continuation.

It still goes on.

5

In the spring of 1975 I was lucky and was given a second chance to work in the Copenhagen archive. Almost everything remained as before. I saw the same house built in the twenties where Bohr had lived, and the same office with the window curtained by the rosy mist though this time it was the spring sun that produced that effect. I saw the same deep steel box filled with the files containing the historical evidence of the veterans, to which new transcripts had since been added. I saw the old curator of the archive who had known practically all of Bohr's collaborators in their younger days, though this time it was not quiet Betty Shulz but another Bohr secretary—the friendly but firm Sophie Hellman. The atmosphere was not so relaxed, perhaps due to the increasing interest in the archive.

Time seemed to have stopped running here as at any place where old documents are stored. But a young man from West Germany read the archive documents at the table where seven years ago I had perused them. The young man was not an author, he was a budding historian and I thought that seven years ago he had been just a boy from Stuttgart and now, as a grown-up historian, he analyzes all the de-

tails of the development of the complementarity principle. I felt a twinge of jealousy knowing that he had a better claim to this table and the quiet of the archive but at the same time I knew that it was only natural to give way to a new generation.

I was allowed to use a light office on the upper floor. Its temporary inhabitant, a theorist from Poland, was away for vacation. I did not know what his subject was but a book in English on crystal chemistry was lying among journals and preprints on a bookshelf. Then I saw two brown volumes that seemed faintly familiar from my old university days. When I opened them I saw the famous *Fundamentals of Chemistry* by Mendeleev (it was the eighth edition published in the thirties)—a surprising book to find in Copenhagen in the seventies, and in the Bohr Institute of theoretical physics at that.

All this together—a German historian, a Polish theorist; a book published in the thirties and opened in the seventies; the great Russian chemist visiting the great Danish physicist; the classical period of one science visiting the anticlassical period of another science; the 19th in the 20th century—all this together seemed to me a living and vivid example of the links in time between peoples and cultures. It was an accidental unexpected example, one of the millions that may and do occur and therefore it felt especially convincing in its natural simplicity.

Human knowledge thrives on such links.

Recently, looking through the notes I made in Copenhagen I came across the note: 'Quote from the Preface to the 8th edition of *Fundamentals of Chemistry* by Mendeleev, p. XXIII...' I recalled how one, evening, tired of reading texts in foreign languages, I had decided to rest awhile by reading in Russian and had picked up Mendeleev's book. Then I

made this note. But what was it I had wanted to quote? I took the book again and found the following words:

> 'At first the sciences, as bridges, could be built only with strong supports and long beams. I would like to show... that for a long time the sciences could be built as suspension bridges supported by a network of cables which could hardly be broken though individually each of the cables could easily snap; in this way ravines could be spanned which had seemed impassable. Thus the sciences learned how to span the ravines of the unknown without touching the bottom, to reach the firm ground of the reality and cover the entire visible world...'

And I thought that this prophetical writing directly concerned our good story, which in fact tells how the quantum bridge was built between the macroscopic world and the submicroscopic world: links could easily be broken one by one but together comprise a secure network...

Then I thought that all this could be said about the advances made in the decades that followed the Sturm und Drang period, when quantum physics having reached the firm ground of reality started indeed to cover the entire visible world. That was when I felt extremely sorry that the archive is silent about that. Time does seem to have stopped in it, it really has come to an end.

I do not reproach the compilers of the archive: they have completed their programme and did it surprisingly well. But our quite understandable interest in the life of remarkable ideas cannot be satisfied by the programme; one wants it extended beyond the

time limits of the first revolutionary period. One would like to see a collection of documentary evidence—and, first of all, evidence submitted by the scientists themselves—describing the entire history of quantum physics up to the present time.

But that is easier said than done. How to compile such an enormous archive? Does it not resemble an attempt to drain the sea with a cup?

There were about thirty people working in the Cavendish laboratory under Rutherford in the twenties; in the same period Bohr started working in his Copenhagen Institute with a staff of seven; Einstein had the services of a secretary only in the late twenties—it is not surprising that the historians could find only about a hundred veterans of the quantum revolution all over the world. But it was not hundreds upon thousands of scientists that continued their work and are working in physics today. So many human destinies, and characters, finds and losses, hopes and disappointments, victories and defeats!

Quantum physics keeps on expanding to cover the entire visible world, branching into many related but separate fields from the quantum field theory (which is of the same age as quantum mechanics itself) to the quantum theory of the cognition processes (the newest subject among the daring theorists). Each branch of the towering tree has its own history of growth. One would wish to establish an archive of documentary evidence for each of the branches.

In our time it would be perhaps the only practicable method for collecting the not-yet-lost historical documents and oral evidence. Thus, what is needed is branching of archives and it would be safer to do that on the national scale; international archives of that type are hardly feasible.

6

One of the larger branches of quantum science is the study of elementary particles.

The year 1932 taken arbitrarily as the last year of the Sturm und Drang period was called by physicists 'the year of miracles'. It was given the name because it added two newcomers to the three known elementary particles (electron, proton, and photon): James Chadwick discovered the neutron predicted by Rutherford, the first known nuclear particle that did not have any charge; and Carl Anderson discovered the positron predicted by Dirac, the first known antiparticle, exactly similar to the electron but with a positive electric charge.

Thus, five particles in three and a half decades of work. But about three decades later one could read in the *Physical Dictionary*: 'The number of elementary particles known by 1965 is considerably higher than 100'! Journalists started talking about a horn of plenty giving forth numerous 'primary entities of matter'. Physicists were also astonished with their new-found abundance; they never had expected it.

First there were two, then four, then six species of neutrinos. Two mu mesons, three pi mesons, four K mesons. The family of nucleons and several broods of particles heavier than nucleons, known as delta resonances. Then lambda hyperons, sigma hyperons, and xi or cascade hyperons. Then sets of even heavier particles—psi or J particles and upsilon particles. They have different masses, different lifetimes, different sets of quantum characteristics, such as the long-known spin and the new-fangled 'strangeness' and 'charm'. They all play different parts in the fundamental physical interactions. Then the astonishing

quarks with fractional electric charges appeared... Now encyclopedias report that in 1978 the number of elementary particles is more than 350!

Indeed, it would seem as if some fantastic horn of plenty were lavishing unheard-of gifts on physicists without any efforts on their part. Yet there is no horn of plenty and the effortlessness is an illusion. The discovery of each new elementary particle is, as a rule, a heroic feat performed first by theorists and then by experimenters. (It is clear beforehand, though, that not each of the new particles is genuinely elementary; frequently they just seem elementary for some time.)

Let me tell about just one such discovery.

It was made in spring of 1960 by the Soviet physicists in Dubna. They demonstrated the existence of the antisigma-minus hyperon that had been predicted in the fifties. This hyperon, like its fellow hyperons, is unstable; its lifetime from birth to decay is a few ten billionths of a second (10^{-10} s).

However, it manages to leave a noticeable trace when shooting through the detector chamber at a velocity close to that of light. Physicists contrive to photograph this trace together with the traces of the particles it decays into. But tens of other submicroscopic events occur in the chamber simultaneously and are also recorded on the photograph thus hiding the only trace that physicists are looking for. Now, at that time computers were not yet employed for such jobs. The experimenters from Dubna themselves had to study 40 000 photographs until they had found the desired trace, and very careful calculations then convinced them that their guest was indeed the antisigma-minus hyperon.

It was the young physicist Anatoly Kuznetsov who first felt that he saw the desired trace. That

was how he described it—he 'felt' it! I wrote about him in my book *The Necessity of the Strange World* where I wanted to portray the psychology of discovery. I did not describe the history of that discovery in detail and therefore it could seem that I gave all the credit for the discovery to one scientist, while actually it was a large group that had worked toward it.

Anatoly Kuznetsov wrote to me that he could 'tell me much that was interesting about his friends who had put great efforts into this work'; he gave the names of ten of his coworkers and two teachers and briefly mentioned the difficulties they had had to overcome. Three years later when American physicists were looking for the omega-minus hyperon predicted by the theorists Gell-Mann and Neeman they had to study traces in 100 000 photographs!

But it was not just the sheer volume of work that was new. At the early dawn of the Sturm und Drang period Marie and Pierre Curie had to perform recrystallization 10 000 times to isolate the first tiny amounts of radium. Geiger and Marsden who worked with Rutherford had had to count 1 000 000 scintillations on the screen produced by alpha particles scattered over different angles when they worked on the substantiation of the concept of the atomic nucleus. Yes, it was something else that was essentially new in physics.

The radium atoms and the atomic nuclei were natural entities. Now the physics of elementary particles learned to create objects for study by using the quantum laws of nature. That is something that physics could not do, and did not need to do in the Sturm und Drang period.

In principle, it is quite simple. The Einstein law of the equivalence of energy and mass—the famous

equation $E=mc^2$—allows transformation of energy into mass (matter) under suitable conditions. Of course, an enormous energy is needed to create a material 'entity' of even a tiny mass. It was observed that a mysterious mechanism gave rise to new particles in the collisions between high-energy particles. Such events constantly take place in cosmic rays but the flux of these rays has too low an intensity and moreover they are uncontrollable. Physicists started to produce high-energy particles in high-power accelerators.

When the group of American physicists headed by Segre and Chamberlain discovered the antiproton in Berkeley in 1955 they produced it in the 6 billion electron-volt accelerator. This energy was more than enough to produce the antiproton whose mass is that of the hydrogen nucleus. Higher energies, however, were needed for producing heavier hyperons and antihyperons. When the Soviet group headed by Vladimir Veksler started work on the antisigma-minus hyperon in 1960 Dubna near Moscow was the only place on Earth where physicists had a source of such energies—the 10 billion electron-volt proton synchrotron commissioned in 1957.

Thus the discoveries of the smallest entities in nature—the elementary particles—necessitated building the largest laboratory instruments in history—the mammoth accelerators. The Soviet physicist Vladimir Veksler had nursed these gigantic machines from their infancy onwards.

Back in 1944 he was the first to formulate the revolutionary principle of acceleration known as the autophasing principle (earlier than the American physicist Edwin McMillan). This principle provided for a thousand-fold increase in the energy of accelerated particles—from millions to billions of electron

volts. Veksler's baby, the accelerator in Dubna, remained for a long time the largest in the world. Later larger accelerators were built in Geneva (Switzerland), Serpukhov (USSR), and Batavia (USA). It was the prototype at Dubna that brought the spectacular success to Veksler's students, and among them Anatoly Kuznetsov.

There is another aspect worth noting. When telling me about the discovery of the antisigma-minus hyperon Kuznetsov particularly emphasized the crucial part played by his coworker Soloviev who designed 'an apparatus without which we would not be able to get photographs of our particle'. This was the Dubna modification of the new high-speed chamber for photographing the track of charged particles: it was not the old cloud chamber invented by Wilson where the particle left a track of fog droplets but a new chamber invented by Glaser where the particle path is traced by bubbles of vapour. It was a highly sensitive complicated apparatus with all the up-to-date automatic equipment (high technology is now a must in physical experiments).

There is another experimental apparatus unknown in the Sturm und Drang period which is indispensable in experiments with particles such as the American discovery of the antiproton in 1955. It is the Cerenkov counter which reports on the important parameters of the particle passing through it which emits the Cerenkov radiation discovered in 1934.

The Soviet physicist Pavel Cerenkov who performed skilful experiments to discover the form of radiation now named after him was a student of Sergei Vavilov who suggested performing the experiments and guided them. The bluish light emitted from a liquid under the effect of a flux of gamma quanta had such interesting properties that it led to a very unex-

pected interpretation: the high-energy quanta made the atomic electrons travel at a speed greater than the speed of light and these electrons produced the bluish radiation trailing after them.

But are not speeds greater than the speed of light forbidden? Yes, of course. But a reminder is in order. It is the speed of light in vacuum that cannot be exceeded while the speed of light in matter—say, liquid or gas—is slower than its speed in vacuum and thus can be exceeded. The theory of the Cerenkov radiation was developed by the Soviet physicist Igor Tamm and Ilya Frank in 1937. More than twenty years later they and Cerenkov were awarded Nobel prizes for that (it was in 1958, Vavilov was no longer alive and Nobel prizes are not awarded posthumously).

At the Nobel prize presentation ceremony in Stockholm Prof. Siegbahn said: 'The discovery of Cerenkov, Tamm and Frank has found applications, in recent years, of decisive importance in the studies of the fundamental structure and nature of matter...' The assistance in the discovery of the antiproton was at the time already on the long list of services rendered to experimental physics by the Cerenkov effect.

The operation of the Cerenkov counter can be easily explained: it records the high-speed particles by their radiation thus allowing scientists to distinguish them from other slower particles. This is why this counter is indispensable in the experiments with gigantic accelerators. Neither is it surprising that the Cerenkov counters are used not only on Earth but in space, too, in the spacecraft laboratories. These latter counters record the data on the particles produced and accelerated not in the laboratories of Dubna (or Berkeley, or Serpukhov or Geneva) but in the Universe where they are accelerated by the greatest accelerator of all—the galactic force fields.

Thus the laboratory apparatus of quantum physics lends assistance to the physics of space.

7

Thus the antisigma-minus hyperon was discovered in Dubna. Continuing Mendeleev's simile for science one can say that this discovery is just one of the network of cables bridging the ravines that had seemed impassable. But how much stands behind this discovery! Just one chapter in the history of elementary particles or even a brief paragraph in it. But how many scientists, famous and obscure, worked for this end for several decades! How great is the contribution of Soviet physicists in the human quest for understanding the fundamentals of nature.

Of course, some day the entire history will be written. But the documentary evidence for it must be collected even now. The letters and oral evidence of veterans are particularly important and cannot be replaced with laboratory diaries, minutes of scientific conferences, or official reports of laboratories. It is in the voices of the veterans that science comes to life before us as the 'drama of ideas' (in the words of Einstein). But veterans depart. The historians will not be able to talk to Vavilov, or Veksler, or Tamm... and you remember how the historians of the Sturm und Drang period came too late to talk to Einstein, Pauli and Schrödinger... Time cannot be stopped.

Perhaps, it is time for historians to remember the old saying and hurry doing good. In fact, this is what any branch of natural sciences needs, not only the branches of quantum physics.

Such were my thoughts during those days spent in the Copenhagen archive.

But how to end our good story which really does not have any end?

Perhaps, it would be best to recall the remarkable words of Mendeleev about sciences that 'learned how ... to reach the firm ground of reality and cover the entire visible world without touching the bottom'. Of course, quantum physics by taking the human mind deep into matter could never 'touch the bottom', it is just looking for it and will look for it forever.

Each new plateau on the plot of growth of quantum physics means a temporary illusion that the bottom has been reached. One recalls the joke of the witty and wise Pole Lezh 'Having descended to the very bottom he heard a knock from below.'

Clearly, the depths of nature are bottomless. The time will come, perhaps soon, when quantum mechanics reaches the bounds of its applicability as happened with classical mechanics in this century. For this to happen the studies of the submicroscopic world have to reach and penetrate a yet unknown level of physical reality. It seems that physicists have already heard 'a knock from below'. At any rate, they keep listening... For instance, there are hints that on a smaller-than-submicroscopic scale the inevitable changes in the geometric properties of space-time will play the decisive role. And this may have revolutionary consequences for the description of nature.

One thing remains clear: the future development and deeper probing of physics will not revive old and familiar classical concepts but will lead to new astonishing surprises, and new great ordeals, out of which will spring the joys of new, yet unforeseen, knowledge.

Further reading

N. Bohr: his life and work as seen by his friends and colleagues, ed. S. Rozental, North Holland, Amsterdam, 1967.

M. Born, *My life and my views,* London, 1968.

D. Danin, *Rutherford,* Molodaya Gvardiya, Moscow, 1967 (in Russian).

D. Danin, *Bohr,* Molodaya Gvardiya, Moscow, 1978 (in Russian).

D. Danin, *Inevitability of the strange world*, Molodaya Gvardiya, Moscow, 1966 (in Russian).

W. Heisenberg, *Physics and beyond*, Allen and Unwin, London, 1971.

M. Jammer, *The conceptual development of quantum mechanics*, N. Y., McGraw-Hill, 1966.

Rutherford by those who knew him, London, 1954.

Rutherford at Manchester, ed. J. Birks, Heywood, London, 1962.

J. J. Thomson, *Recollections and reflections,* London, 1936.

V. Weisskopf, *Knowledge and wonder,* Doubleday, N. Y., 1962.

R. Clark, *Einstein, life and times,* World Publ. Co., N.Y., 1971.

M. Born, *My life: recollections of a Nobel laureate,* Scribners, N.Y., 1978.

TO THE READER

Mir Publishers would be grateful for your comments on the content, translation and design of this book.

We would also be pleased to receive any other suggestions you may wish to make.

Our address is:
Mir Publishers.
2 Pervy Rizhsky Pereulok.
I-110, GSP, Moscow, 129820, USSR.

Printed in the Union of Soviet Socialist Republics

Other Mir Titles

SPACE ADVENTURES IN YOUR HOME

F. Rabiza

It might appear that your cosy home is a far cry from spac
with its unfathomable expanses and huge spectacular project
But this book will usher you into the fascinating world o
space without forbidding rockets and aerospace labs. Th
book is crammed with make-it-yourself ideas and instructiv
experiments made using bits and pieces from the cellar, th
lumber-room or the kitchen cupboard. You will learn how t
make exciting things such as jet vanes, Segner wheel, in
pulse ship model, micromodels of guided "spaceships", et
with the projects becoming ever more sophisticated and spe
tacular. With this book you will learn much and experien
much. Everything has its beginning... Maybe this book wi
become a first step of the tramp of a spacecraft.
The book may be useful for a wide range of readers.

This absorbing account of the 'quantum resolution' in physics introduces the reader to the fascinating concepts of probability and relativity - and to the men and women whose successes and disappointments in their work made it possible.

The term 'quantum' first appeared at the very beginning of the century, on December 14th 1900. Its reluctant coiner, Max Planck, considered it 'only a working hypothesis'. In a few years time Einstein was to declare, 'the quantum exists!' Yet the development of quantum mechanics was not a steady series of triumphs: as perhaps never before in the history of science, dramatically new ideas were paralleled by dramatic events in the lives of those who created the new physics.

The author (a well-known Soviet writer on science) presents an intriguing story that draws on the priceless taped interviews with the pioneers of this revolution that are preserved in Copenhagen; the book will engage the interest and emotions of the scientific and general reader alike.